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CONTENTS

On the Nature of Viruses, Cancer, Genes, and Life—A Declaration of Dependence. (*Penrose Memorial Lecture.*) WENDELL M. STANLEY 317

Comments on Viral Cytopathogenicity. JOHN F. ENDERS 325

Repeated Plasmapheresis in the Same Person. A Rationale for Modern Bloodletting. JOSEPH STOKES, JR., and JOSEPH SMOLENS 330

Influence—Past and Present—of François-André Michaux on Forestry and Forest Research in America. J. R. SCHRAMM 336

André and François-André Michaux and their Predecessors. An Essay on Early Botanical Exchanges between America and France. GILBERT CHINARD 344

French Botanists and the Flora of the Northeastern United States: J. G. Milbert and Elias Durand. WILLIAM J. ROBBINS 362

Jamestown, 1607–1957. THOMAS J. WERTENBAKER 369

Iceland and its Literature. STEFÁN EINARSSON 375

Nestor's Pylos. CARL W. BLEGEN 379

The Nation's Research and Development—Their Deficiencies and Means for Correction. MERVIN J. KELLY 386

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PENROSE MEMORIAL LECTURE

ON THE NATURE OF VIRUSES, CANCER, GENES, AND LIFE— A DECLARATION OF DEPENDENCE

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(*Read April 25, 1957*)

EACH of the four topics mentioned in the title of this lecture is substantial enough to warrant having an entire lecture devoted to it alone. Actually a proper and full discussion of viruses, of cancer, of genes, or of life would require many hours. It may, therefore, appear quite presumptuous to have included all four in the title of a single lecture. But let me hasten to indicate that I do not propose to attempt to develop these topics as such, but that I do propose to sketch in certain basic information and then to devote most of my time to a discussion of new relationships between these four subjects, relationships which I believe to be of the utmost importance.

Recent scientific discoveries, especially in the virus field, are throwing new light on the basic nature of viruses and on the possible nature of cancer, genes, and even life itself. These discoveries are providing evidence for relationships between these four subjects which indicate that one may be dependent upon another to an extent not fully appreciated heretofore, and hence the time is appropriate for a declaration of the nature of the dependence that may be involved. Too often one works and thinks within too narrow a range and hence fails to recognize the significance of certain facts for other areas. Sometimes the important new ideas and subsequent fundamental discoveries come from the borderline areas between two well-established fields of investigation. I trust, therefore, that this declaration of dependence will result in the synthesis of new ideas regarding viruses, cancer, genes, and life, and that these ideas in turn will result in the doing of new experiments which may provide the basis for fundamental discoveries in these fields which are so important to every one of us.

Now I suppose there is no doubt but that, of the four topics, life is the one that most people would consider to be of the greatest importance. One would think that the nature of life would be easy to define since we are all experiencing it. However, just as life means different things to different people, we find that in reality it is extremely difficult to define just what we mean by life or by a living agent in its most simple form. There is no difficulty in recognizing an agent as living or non-living so long as we contemplate structures such as man, cats, and dogs, or even small organisms such as the bacteria, or, at the other extreme, structures such as a piece of iron or glass, an atom of hydrogen, or even a molecule of water, sugar, or of our blood pigment, hemoglobin. The former are examples of animate or living agents whereas the latter are examples of inanimate or non-living things. But what is the true nature of the difference between a man and a piece of iron, or between a bacterial organism and a molecule of hemoglobin? The ability to grow or reproduce and to change or mutate has long been regarded as a special property characteristic of living agents. Certainly mankind and bacteria have the ability to assimilate and metabolize food, respond to external stimuli and to reproduce their kind—properties not shared by bits of iron or by molecules of hemoglobin. Now if viruses had not been discovered, all would have been well. The organisms of the biologist would have ranged from the largest of animals, whales and elephants and the like, all the way down to the smallest of the bacteria which are about 200 mμ or a few millionths of an inch in diameter. There would have been a definite break with respect to size since the largest molecules known to the chemist were less than 20 mμ in size. Life and living agents would have been represented solely by those structures which possessed the ability to reproduce themselves and to change or mutate and all of these were about 200 mμ or larger in size, thus more than ten times larger than the largest known molecule. This would have provided a comfortable area of separation or discontinuity between living and non-living things and would have provided ample justification for considering life as some-

thing set distinctly apart and perhaps unapproachable and unexplainable by science.

Then around 1900 came the discovery of the viruses—first the plant virus of tobacco mosaic, then foot-and-mouth disease virus of cattle, and then the first virus affecting man, namely, yellow fever virus. These infectious, disease-producing agents are characterized by their small size, by their ability to grow or reproduce within specific living cells, and by their ability to change or mutate during reproduction. Their inability to grow or reproduce on artificial or non-living media did not cause too much concern and their reproductive and mutative powers were enough to convince most people that viruses were merely still smaller ordinary living organisms. However, around 1930 the sizes of different viruses were determined with some precision, and it was found that some viruses were indeed quite small, actually smaller than certain protein molecules. Then in 1935 the first discovered virus, tobacco mosaic, which is a middle-sized virus, was isolated in the form of a crystallizable material which was found to be a nucleoprotein, that is, a substance composed of nucleic acid and protein. This nucleoprotein molecule was found to be 15 mμ in cross section and 300 mμ in length and to possess the unusually high molecular weight of about fifty million. It was, therefore, larger than any molecule previously described, yet it was found to possess all of the usual properties associated with larger protein molecules. The same material could be obtained from different kinds of mosaic diseased plants such as tomato, phlox, and spinach plants, whereas plants diseased with different strains of tobacco mosaic virus yielded slightly different nucleoproteins. Many tests indicated that the new high molecular weight nucleoprotein was actually tobacco mosaic virus and it was concluded that this virus could, in fact, be a nucleoprotein molecule. Here, therefore, was a molecule that possessed the ability to reproduce itself and to mutate; hence, the distinction between living and non-living things which had existed up to that time seemed to be tottering and soon a full scale intellectual revolution was in progress.

Today the revolution is past and we know that the gap between 20 and 200 mμ has been filled in completely by the viruses—so much so that there is actually an overlapping with respect to size at both ends. Some larger viruses are larger than certain well accepted living organisms whereas some small viruses are actually smaller than certain protein molecules. We have, therefore, a continuity with respect to size as we go from the electrons, mesons, atoms, and molecules of the physicist and the chemist, to the organisms of the biologist and on, if you please, to the stars and galaxies. Nowhere is it possible to draw a line in this continuity of structures and say that all above this size are living and all below are non-living. There appears to be a gradual transition with respect to size and complexity of structure as one goes from things that are normally considered to be alive to things that are generally considered to be non-living. One is reminded of the quotation attributed to Aristotle over 2,000 years ago to the effect that Nature makes so gradual a transition from the animate to the inanimate that the boundary line between the two is doubtful and perhaps non-existent. Much scientific knowledge has been accumulated since Aristotle's time but the essence of his statement is as true today as it was when he made it. But does this mean there is really no difference between the animate and the inanimate? I do not believe that it does. However, we must be willing to define what we mean by life and then we must be willing to accept as living any structure possessing properties fulfilling such a definition.

The essence of life is the ability to reproduce. This is accomplished by the utilization of energy to create order out of disorder, to bring together into a specific predetermined pattern from semi-order or even from chaos all of the component parts of that pattern with the perpetuation of that pattern with time. This is life. Now there is another very basic property which seems to be characteristic of living things and that is the ability to mutate, to change or to respond to a stimulus. I do not believe this property is absolutely necessary for life, but it certainly lends grandeur to life, for not only is it responsible for the whole evolutionary process and thus for the myriads of kinds of life we have on earth but, most importantly for mankind, it permits one to dare to aspire. It is presumably responsible for man, his conscience and his faith. It is obvious that I believe that mutation merits much, much study.

The discovery of viruses has permitted us to contemplate the nature of life with a new understanding. It has enabled us to appreciate in a new light the inherent potentialities of chemical structure, whether that of a single molecule or that produced by the interaction of two or more molecules. Viruses were discovered by virtue of their ability to replicate and in the last analysis this ability to reproduce remains today as the only

definitive way in which they can be recognized. We may purify and isolate preparations from virus diseased tissues but it is only when a reasonably pure material is obtained and units of this are found to possess the ability to reproduce themselves that we are privileged to refer to the material as virus. Since the isolation of tobacco mosaic virus in the form of a crystallizable nucleoprotein 15 by 300 mμ in size, many other viruses have been obtained in pure form and characterized in part by their chemical and physical properties. My colleagues, Arthur Knight, Robley Williams, and Howard Schachman, have made major contributions to the biochemical, electron microscopical, and biophysical knowledge of viruses. Until two years ago all viruses studied had been found to be at least as complex as a nucleoprotein. However, some appear to have lipid, carbohydrate, and in some cases a limiting membrane in addition to nucleic acid and protein. Whereas some viruses, like tobacco mosaic, are crystallizable nucleoproteins which have the usual molecular properties, other viruses, such as vaccinia, have a degree of morphological differentiation which can hardly be called molecular in nature and which is rather more organismal or cell-like in nature. Some of the bacterial viruses have a very complex morphology, with a head and a tail somewhat similar to the sperm of higher organisms.

For a long time many investigators thought that the plant viruses differed basically from viruses affecting animals and man. This idea stemmed mainly from the fact that for twenty years all of the crystallizable viruses were plant viruses. This idea had to be relinquished two years ago when my colleagues, Carlton Schwerdt and Frederick Schaffer, obtained poliomyelitis virus, which is a typical animal or human virus, in crystalline form. Since then at least one other animal or human virus has been crystallized and this is crystalline Coxsackie virus obtained by Doctor Mattern of the National Institutes of Health. Hundreds of viruses are known and more are being discovered every month; yet only a dozen or so have been obtained in purified form. In view of the possibility that these may represent the more stable and more readily purified viruses, one cannot be certain that a true picture of the chemical and physical properties of viruses as a whole has been obtained as yet. However, I believe that we have sufficient sampling to be significant for the purposes of the present discussion for we already know that viruses may range from small crystallizable animal, human, or plant viruses which are nucleoprotein molecules, through intermediate structures consisting of nucleoprotein, lipid, and carbohydrate, to large structures possessing a morphology and composition similar to that of accepted cellular organisms. All of these diverse structures are bound together by one all-important property, that of being able to reproduce their own characteristic structure when placed within certain living cells. They are all, in short, by definition, alive.

Now I am only too fully aware of objections that some may have to considering a crystallizable nucleoprotein molecule as a living agent. Some may feel that life is a mystery which is and must remain beyond the comprehension of the human mind. With these I must disagree. Some may believe that a living molecule is contrary to religion. Here again I must disagree for I see no conflict whatsoever between science and religion and I see no wrong in accepting a molecule as a living structure. To many scientists the diverse expressions of chemical structure represent miracles, and our expanding knowledge of the wonders of nature provides ample opportunities to express our faith and only serves to make us full of humility. Some may prefer to regard a virus molecule in a crystal in a test tube as a potentially living structure and to restrict the term "living" to a virus during the time that it is actually reproducing. I would have no serious objection to this for I am reminded of the facts that certain tapeworms a foot or so in length can live and reproduce only in certain hosts and that even man himself can be regarded as requiring rather special conditions for life, yet no one objects to accepting man and tapeworms as examples of life. I am also reminded that we are taught that the essence of a thing is not what it is, but what it does, and the doing of something involves time; hence there may be good reason always to consider the virus with time. Regardless of certain mental restrictions that may differ from person to person, I think there is no escape from the acceptance ultimately of viruses, including the crystallizable viral nucleoprotein molecules, as living agents. This must be done because of their ability to reproduce or to bring about their own replication. Certainly the essence of life is the ability to reproduce, to create a specific order out of disorder by the repetitive formation with time of a specific predetermined pattern and this the viral nucleoprotein molecules can do.

Of course, it would have been dull indeed if

the first formed living agent had been restricted to exact duplicates of itself. The logical reasoning provided in schemes such as those outlined by Calvin, Haldane, Horowitz, Oparin, and Urey by means of which relatively complex organic substances could have arisen from inorganic matter provides justification for assuming that a chemical structure, perhaps something like nucleic acid, which possessed the ability to replicate, did come into being once upon a time. It needed to have happened only once and thereafter without the great phenomenon of mutation it merely would have kept going until it had filled the world with replicates of this precise structure or until it had exhausted the starting materials. However, Nature has provided a built-in error so that the replication process is not perfect and about one in every million or so replicates is slightly different. This change, which has been of tremendous fundamental importance, we now recognize as mutation and as these errors or differences were accumulated by replicating structures it became necessary to make formal recognition of them. These differences or markers we now call genes. We do not recognize genes directly but only by differences. Needless to say, some physical structure had to be responsible for the accumulation, preservation, and potential exhibition of these differences and this assembly of genes we call a chromosome. The incorporation of one or more assemblies of genes into a structure possessing a limiting membrane, which we now call a cell, then made possible gene interchanges between these cellular assemblies. This genetic interchange by the fusion of two cells, a sexual process, also represents a phenomenon of the greatest fundamental importance for this permitted genetic recombination, a factor that has served to speed up the evolutionary process immeasurably. Therefore, life as we know it today is dependent not only upon reproduction but also upon mutation and genetic recombination.

Now let us consider for a moment the relationships between genes and viruses since we see that both are related to life. Muller's estimate of the maximum size of a gene would place it just below tobacco mosaic virus, near the middle of the viruses. Both genes and viruses seem to be nucleoproteins and both reproduce only within specific living cells. Both possess the ability to mutate. Although viruses generally reproduce many times within a given cell, some situations are known in which they appear to reproduce only once with each cell division. Genes usually reproduce once with each cell division but here also the rate can be changed, as, for example, in the case of polyploidy resulting from treatment with colchicine. Actually the similarities between genes and viruses are so remarkable that viruses very early were referred to as "naked genes" or "genes on the loose." Two great discoveries, one which began in 1928 and the other which occurred in 1952, have provided experimental evidence for an exceedingly intimate relationship between viruses and genes. In 1928 Griffith found that he could transform one specific S type of pneumococcus into another specific S type by injecting mice with nonvirulent R forms together with large amounts of heat-killed S pneumococci of a type other than that of the organisms from which the R cells were derived. Living virulent S organisms of the same type as the heat-killed S forms were then recovered from the animals. Later Dawson and Sia as well as Alloway found that the addition of an extract of one type of capsulated pneumococcus to a culture of a noncapsulated rough form would convert the latter into the same type of capsulated pneumococcus which provided the extract. It was obvious that something was being transferred and in 1938 I discussed the possibility that this "something" might be a virus. In 1944 Avery and his colleagues at the Rockefeller Institute proved that this something was a transforming principle consisting of deoxyribonucleic acid (DNA). Muller in 1947 discussed the possibility that the DNA might correspond to still viable parts of bacterial chromosomes loose in solution which, after entering the capsuleless bacteria, undergo a kind of crossing over with the chromosomes of the host, but this suggestion was not widely accepted. That the phenomenon was not an isolated one was demonstrated in 1953 by Leidy and Alexander who obtained similar results with an influenza bacteria system. The close relationship to genetics was further emphasized by work of Hotchkiss and by Ephrussi-Taylor who, as well as Leidy and Alexander, showed that drug resistance and other genetic factors could be so transferred. This work provided evidence that genetic factors or genes, if one prefers such a designation, can be represented by DNA and can be obtained in chemically pure solution.

This information as well as our knowledge of viruses was soon fortified by the very important discovery by Zinder and Lederberg in 1952 of transduction in Salmonella by means of a bacterial virus. It was found that genetic factors

could be carried from one type of Salmonella cells to another type by means of a bacterial virus. In this type of transformation the genetic fragment is not free but is carried within the structure of the bacterial virus. It is, for example, not affected by the enzyme deoxyribonuclease, and in this respect is unlike the DNA pneumococcus transforming principle. However, it is not necessary for the virus actually to possess virus activity, for killing of the virus by ultraviolet light does not prevent the transduction of other traits. The closeness of the relationship between the virus and the genes of the host is emphasized by the fact that the transducing ability of any bacterial virus is determined strictly by the character of the cells on which the virus was most recently grown. Virus grown on Serotype E_2 Salmonella cells will, when added to Serotype E_1 cells, convert a fraction of these cells into Serotype E_2 cells. It is of interest to note that the virus in filtrates of toxin-forming bacterial strains will convert non-toxin-forming cells into toxin-forming cells. In transduction, a fragment of a chromosome which might be regarded as a gene or a collection of a few or even many genes can be transferred from one kind of donor cell to another kind of receiver cell and be incorporated into the genetic apparatus of the receiver cell. In the pneumococcus or influenza bacterium this can be caused by a DNA preparation which can be separated and isolated as such and in Salmonella this gene or gene collection rides within the bacterial virus, presumably with the viral DNA, which is added to the cell to be transduced. Here one hardly knows what to call a virus and what to call a gene for it is obvious that at times the two merge completely.

The persistence of a bacterial virus in an apparently concealed form or prophage in lysogenic strains of bacteria, extensively investigated by Lwoff, provides further evidence in this direction. Lysogenic bacteria perpetuate in what may be considered a hereditary manner the property of being able to produce a bacterial virus. The term "prophage" is used to describe the form in which the potentiality to produce a bacterial virus is perpetuated in lysogenic bacteria. Prophage is non-pathogenic and non-infectious in the usual sense, but, since it is multiplied at least once with each cell division, it may be regarded as infectious in the sense that genes or chromosomes are infectious. In other words, the prophage might be considered as a temporary part of the genetic apparatus of the cell, the genetic element that differentiates a lysogenic from a sensitive cell, and at the same time as the non-infectious form of a bacterial virus. There are times, therefore, when a virus may not exhibit its normally infectious nature but have its potentially unlimited reproductive capacity under genetic control so that it replicates only once with each cell division. There are times when a specific genetic element of a cell can be freed of the normal controlling mechanism of the cell and go forth in viable form in solution or associated with a virus, enter a different cell, replace a homologous chromosomal segment and resume its original specific function in the new cell. It is obvious that the latter phenomenon could readily be considered an infectious process, and that viruses can act as genes and genes as viruses under certain circumstances.

I should now like to discuss the relationships which involve cancer. You probably know that cancer or abnormal, uncontrolled cellular growth may occur in all kinds of organisms and that cancer is second only to heart disease as a killer of mankind; hence I need say no more about the relationship between cancer and life. Cancer originates when a normal cell for reasons, some known and some unknown, suddenly becomes a cancer cell which then multiplies wildly and without apparent restraint. Cancer may originate in many different kinds of cells, but the cancer cell usually continues to carry certain traits of the cell of origin. The transformation of a normal cell into a cancer cell may have more than one kind of a cause, but there is good reason to consider the relationships that exist between viruses and cancer. Viruses have been implicated in animal cancers ever since Peyton Rous, in 1911, transmitted a chicken sarcoma from animal to animal by means of a cell-free filtrate. Despite the fact that today viruses are known to cause cancer or tumors in chickens, pheasants, ducks, mice, frogs, rabbits, deer, and other animals, and even in certain plants, there exists a great reluctance to accept viruses as being of etiological importance in human cancer. However, basic biological phenomena generally do not differ strikingly as one goes from one species to another, and I must say that I regard the fact, now proved beyond contention, that viruses can cause cancer in animals to be directly pertinent to the human cancer problem. It should be recognized that cancer is a biological problem and not a problem that is unique for man.

Since there is no evidence that human cancer as generally experienced is infectious, many persons believe that because viruses are infectious agents they cannot possibly be of etiological im-

portance in human cancer. However, this is not a valid conclusion for several reasons. It is well known from the work of Bryan and of Beard that animal cancer viruses may alternately be filterable and hence infectious and then non-filterable and hence appear non-infectious, apparently owing to great variations in the actual amount of virus present in the cancer. It is also well known that viruses may be highly specific, so specific in fact that a given virus may infect and cause disease only in one kind of cell in one kind of animal and hence, under all other conditions, appear non-infectious. For example, the kidney carcinoma virus of the leopard frog studied in this city by Lucké would appear to be such a virus. Then there is the possibility that man may be carrying viruses of etiological importance for cancer which for one reason or another have not yet been discovered. The possibility of mutation of latent viruses into a new strain of etiological importance must also be kept in mind. Pertinent to both of these possibilities is the discovery during the past few years of dozens upon dozens of hitherto unknown viruses in human beings. These consist of the ECHO viruses isolated from the human intestinal tract, the adenoviruses isolated from the upper respiratory tract and eyes of man and a group of viruses isolated from human sera. New viruses of man are discovered almost every week. Thus we now have many more human viruses than we know what to do with and there is no reason to shy away from giving consideration to viruses as causative agents in human cancer for lack of the viruses.

During the past few years there has been an almost unbelievably rapid development of techniques by means of which it is now possible to grow almost all kinds of human and animal cells in the test tube and, as a consequence, vast new opportunities for experimentation on human cells without danger to man have opened to us. These cells are also providing a means for the isolation of new viruses, since many kinds of cells are very susceptible to many viruses. The human amnion cell, which my colleagues Elsa Zitcer, Jørgen Fogh, and Thelma Dunnebacke first obtained from the full term amnion in cell culture, is proving of great use in this connection as well as in studies on the transition from a normal to a potentially malignant cell. For example, we are finding interesting changes in chromosome number and in ability to grow in cortisone or x-ray treated animals as these human amnion cells are passed in culture. It is also of interest that one of the adenoviruses has been found to destroy human cancer cells both in the human being and in the test tube. Thus a virus may cause a cancer and a virus may destroy a cancer. Unfortunately in the case of Huebner's studies on carcinoma of the human cervix not all of the cancer cells were destroyed and the cancer eventually progressed. However, Huebner, as well as others, is attempting to train a series of viruses to grow on cancer cells, so this approach may not be too hopeless. In the same way it is possible to train cells to respond to viruses and this may provide even better test systems for human viruses as yet undiscovered. Even if eventually one should find no cancer virus among the large number of human viruses, the fact that man carries so many viruses within his cells and that these are continually passing from person to person means that we should be ever alert to the possibility of transduction by these viruses. Of course, there is no confirmed case of transduction in higher organisms as yet. However, human cancer is a fact and there is certainly something within every human cancer cell that insures its reproduction whether we call it a gene or a chromosomal fragment, and so long as human viruses are so abundant we certainly have the possibility of transduction.

There are many examples of latent viruses that may remain hidden for a lifetime or even for generations only to come to light as a result of some treatment or change. Most human beings acquire the virus of herpes simplex quite early in life and in many persons the evidence for the persistence of this virus throughout their lifetime is quite good. Traub has found that infection of a mouse colony with the virus of lymphocytic choriomeningitis can result, with time, in an inapparent infection of all animals. The virus is apparently transmitted *in utero* and remains with the animal throughout its life; hence this virus persists throughout generation after generation of mice. Injection of such mice with sterile broth can revive the pathogenicity of the virus and bring it into light. Certain potato viruses such as potato X virus, also known as the healthy potato virus or the latent mosaic of potato virus, can be passed from generation to generation without causing an apparent disease. This virus is not present in several varieties of potato grown in Europe, but it is thought to be present in all, or almost all, potato plants grown in the United States. Needless to say, it was only by virtue of the fact that potato plants without this virus are known to exist and the fact that this virus causes obvious

disease symptoms when inoculated to certain other plants that it was possible to establish the actual existence of this virus. In the absence of this information this latent mosaic virus would have to be regarded as a normal constituent of the potato plant.

Since viruses can mutate and examples are known in which a virus that never kills its host can mutate to form a new strain of virus that always kills its host, it does not seem unreasonable to assume that an innocuous latent virus might mutate to form a strain that causes cancer. The great wealth of newly discovered viruses of man plus our knowledge of the latent virus phenomenon provides ample justification to reexamine quite carefully the relationships between viruses and human cancer.

Another fact which may prove of the greatest importance in this connection is that treatment of certain lysogenic strains of bacteria with physical and chemical agents, such as x-rays, ultraviolet light, nitrogen mustard, certain chemical-reducing agents or iron-chelating agents, results, after a latent period, in the lysis of the bacterial cells and the release of large amounts of bacterial virus particles. These agents are called "inducers" and you may recognize some as carcinogenic agents for man and animals. Non-lysogenic bacteria are unaffected by these "inducers" in so far as the production of a bacterial virus is concerned. Is it possible that this activation of a prophage by certain chemical or physical agents with development into a fully infectious bacterial virus and the consequent destruction of the bacterial cells provides a biological example of a process which occurs in man? I believe that this activation of prophage as well as the phenomenon of transduction by free deoxyribonucleic acid in the pneumococcus and by bacterial viruses in Salmonella is pertinent to the human cancer problem, especially so in view of the recent discovery of dozens upon dozens of new viruses of man. Certainly the experimental evidence now available is consistent with the idea that viruses, as we know them today, could be the etiological agents of most, if not all cancer, including cancer in man. I have been urging the acceptance of this idea as a working hypothesis because it will result in the doing of experiments that might otherwise be left undone, experiments that could result in the solving of the cancer problem. Needless to say, what we do in the way of experimentation depends in large measure upon what we think and I am sure the time has come when we should change our thinking with respect to the nature of cancer.

I hope that by this time it is obvious that viruses, cancer, genes, and life are tied together by a whole series of relationships, that viruses can act as genes and genes as viruses under certain circumstances, that viruses can cause cancer and that viruses are structures at the twilight zone of life partaking both of living and of molecular properties. Let us now see whether there is a common thread of understanding permeating all of these relationships. We know that viruses have been thought to be at least as complex as a nucleoprotein, but we also know that the transforming agent of the pneumococcus has been found to be a deoxyribonucleic acid and there is presumptive evidence that the genetic stuff of the bacterial viruses is also deoxyribonucleic acid. However, until recently no gene or chromosome or any of the ordinary viruses had been isolated as such in the form of nucleic acid; hence the "stuff of life," as well as the viruses, has been considered to be nucleoprotein in nature with considerable doubt as to whether the protein or the nucleic acid or the combination of the two was really the biologically active structure.

A recent very important discovery made in our laboratory by Doctor Fraenkel-Conrat has changed the situation considerably and now makes it seem certain that nucleic acid is the all-important structure. It was reported by Fraenkel-Conrat and also shortly thereafter by Gierer and Schramm in Germany that special treatment of tobacco mosaic virus yielded a nucleic acid preparation possessing virus activity. It would now appear necessary to recognize that a nucleic acid structure of around 300,000 molecular weight can possess coded within its 1,000 or so nucleotides not only all of the information that is necessary to bring about in the host cell the production of more of this same nucleic acid, but also apparently the *de novo* synthesis of its own characteristic and highly specific protein with which it eventually coats itself. This work provides wonderful evidence for a direct relationship between specific nucleic acid and specific protein synthesis and makes it possible to consider virus and gene action, including their relationships to cancer and to the nature of life, in terms, not of nucleoprotein structure, but in terms of nucleic acid structure. We see, most importantly, that viruses, cancer, genes, and life are all directly dependent upon the structure of nucleic acid.

It may be calculated that a thousand-unit poly-

nucleotide linear chain consisting of a coded repeat of only four different components, adenine, guanine, cytosine, and uracil, in the same ratio as exists in tobacco mosaic virus nucleic acid could form about 10^{590} different arrangements. This number is so large that it is incomprehensible. Even a one hundred-unit polynucleotide chain of this composition could exist in about 10^{57} different arrangements and this number is vastly larger than the total of all living things on earth and in the oceans. We have, therefore, in this structure consisting of the four chemicals, adenine, guanine, cytosine, and uracil (thymine in the case of deoxyribonucleic acid), repeated many times over in unique fashion the code for every bit of life on earth and in the sea. When a normal cell becomes a cancer cell there is undoubtedly a change in this structure within the cell. It is of interest to note that many anti-cancer compounds are antimetabolites for these chemical components of nucleic acids. And in our laboratory Litman and Pardee made the very important observation that the incorporation of 5-bromouracil into a bacterial virus in place of thymine resulted in the production of the highest percentage of mutants ever recorded. Certainly all of this information plus the discovery that virus activity can be a property of nucleic acid and our knowledge of relationships between viruses, cancer, genes, and life now make it obvious that the common thread upon which all of these depend is specific nucleic acid structure. Therefore, this declaration of dependence revolves around nucleic acid.

I believe that the elucidation of the structure of nucleic acid in all of its aspects is the most important scientific problem we face today. It is vastly more important than any of the problems associated with the structure of the atom, for in nucleic acid structure we are dealing with life itself and with a unique approach for bettering the lot of mankind on earth. It is possible that the solution of this scientific problem could lead eventually to the solution of major political and economic problems. Never before has it been possible to realize so fully our utter dependence upon the structure of nucleic acid. Eventually chemists should be able to synthesize a small polynucleotide specifically arranged; hence one may now dare to think of synthesizing in the laboratory a structure possessing genetic continuity and of all of the tremendous implications of such an accomplishment.

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COMMENTS ON VIRAL CYTOPATHOGENICITY

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(*Read April 25, 1957*)

INTRODUCTION

THE multiplication of viruses, so far as is known, is dependent upon an extremely intimate association with living cells of higher forms. During the course of viral multiplication, which takes place within the cell, the cell is frequently, although not invariably, injured or destroyed. This capacity of viruses to injure and destroy cells, i.e., to cause cellular disease, is now often referred to as "viral cytopathogenicity," and the resultant changes are designated as "cytopathogenic effects." Of these effects some are more obviously characteristic than others and these, for many years, have been accepted by animal and plant pathologists as presumptive evidence of certain viral infections. Changes particularly suggestive of such infections take the form of amorphous or crystal-like masses in cytoplasm or nucleoplasm. These masses are termed inclusion bodies and examples will be presently given. Others somewhat less distinctive consist in swelling or "ballooning" of the cell caused by certain of the pock-forming viruses like those of vaccinia and chicken pox, or in the formation of multinuclear giant cells induced by the virus of measles. These various changes are usually, if not inevitably, followed by more profound disruption of structure terminating in the death and disintegration of the cell.

Not all species of viruses, however, produce effects so characteristic. Many, which include the polio virus, the Coxsackie viruses and the newly recognized ECHO group induce cytologic aberrations that are in part non-specific in character. Of these the most common are nuclear pyknosis, loss of cytoplasmic detail with increased affinity for acid dyes and eventual disruption of the entire cell. Yet even viruses of this class also exhibit the capacity to produce alterations that are more specific such as peculiar deformations of the normal outline of the cell, increasing refractility of the cytoplasm, and unusual condensations of nuclear or cytoplasmic constituents.

In viral diseases of multicellular organisms it is evident that the cytopathogenic effect represents the fundamental injury from which stem many, although probably not all, of the other pathologic and clinical manifestations, such as hyperplasia, abnormal growth and disturbances of nutritive, muscular, or nervous function. In the case of viruses that attack unicellular organisms, of which only the bacteriophages are known at present, the disease is necessarily represented solely by the cytopathogenic effect which ends in the rupture and lysis of the bacterial cells.

For a long period the investigation of viral cytopathogenicity in multicellular organisms was, with few exceptions, confined to observations of changes occurring in infected tissues of plants and animals. In this way, much of value was learned concerning the appearance and significance of disturbances caused by these agents acting, as it were, in their natural environment. Clearly, however, in a milieu of such complexity, there was little hope of making, under controlable conditions, the biologic and biochemical analyses that will be required to define precisely the nature of those factors in virus and cell, which, reacting together, almost certainly will be found to underlie the observed morphological abnormalities.

The effective approach to this important problem demands first of all greatly simplified systems in which multiplication of the virus will take place. Ideally such a system would comprise only the virus and a quantitatively determinable population of one kind of susceptible cell maintained *in vitro* by a nutritive medium of known composition. Systems of this sort have long been available for researches on bacterial viruses and have been exploited effectively by numerous investigators. As a result we now know much more in detail about the reactions between this group of agents and their host cells than we do about those of animal or plant viruses. Even so, the mechanism whereby the phage brings about the destruction of the bacterial cell has not been clearly discerned.

Contrasting with this situation respecting bacteriophages, it is only recently, through advances

made in the techniques of tissue culture, that systems have been devised in which reasonably satisfactory analyses of the interactions between many of the animal viruses and their host cells can be carried out. At present these tissue culture techniques are being extensively used by a number of workers in researches on the cytopathogenicity of the large group of animal viruses. In the case of plant viruses, application of the tissue culture method to the investigation of cell-virus relationships has been greatly limited by the barriers to viral infection imposed by the impenetrable nature of the plant cell membrane. But we may be confident, I think, that ways will be found to overcome this difficulty.

CYTOPATHOGENICITY AS ILLUSTRATED BY THREE VIRUSES RECENTLY IDENTIFIED

With this much as background, I shall, during the remainder of this paper, illustrate the cytopathogenicity of viruses by describing the effects produced in cultivated cells by three agents that have only recently become available for study in the laboratory, and conclude with a summary of the general features of this property.

I have chosen as the first illustration the changes that occur in human epithelial cells infected by the virus of measles. These changes were first observed by Enders and Peebles [1] in 1954 following the addition to tissue cultures of blood and throat washings taken from patients during the acute phase of the disease. The cultures employed contained human kidney epithelial cells. A few days after the virus was introduced the regular sheet-like arrangement of these cells was seen in places to be disturbed. In these areas, cell boundaries were no longer distinguishable and on closer examination it was noted that the cytoplasm of the adjacent cells had apparently fused. Distributed irregularly throughout the large expanse of cytoplasm thus established, were seen a varying number of nuclei. In stained preparations, eosinophilic inclusion bodies, characteristic of many viral infections, were conspicuous in nearly all of the nuclei and irregular red stained masses, or inclusions, were found scattered in the cytoplasm. The appearance of these cellular aggregates mimic rather closely the multinuclear giant cells found in the tonsils, appendix, and other lymphoid structures in patients with measles and so, for the sake of convenience, we shall hereafter speak of them as giant cells. Essentially the same manifestations of cellular injury appeared when the virus was first transferred from the kidney cell cultures to those composed of epithelial cells obtained from human amniotic membranes. In figure 1 is shown the normal appearance of these cells magnified 572 times, in a culture without virus. Figure 2 illustrates an affected area in a companion culture exposed sixteen days previously to the measles agent. Eosinophilic intranuclear inclusions are conspicuous features and the red-staining masses within the cytoplasm are clearly defined.

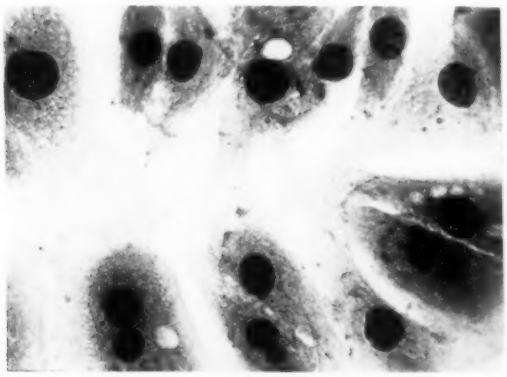

FIG. 1. Appearance of human amnion cells, growing in tissue culture. No virus added. H & E stain. Mag. 572 ×. Note clearly defined cell boundaries.

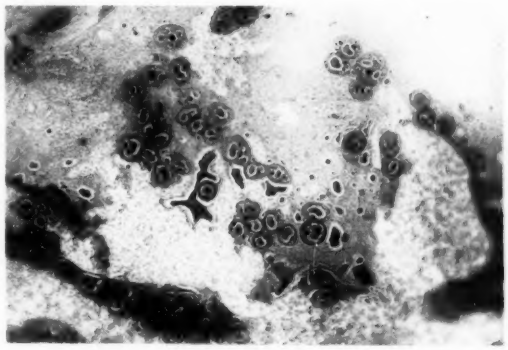

FIG. 2. Multinuclear giant cell formation produced by measles virus (Edmonston strain) in a culture of human amnion cells maintained in same manner as those shown in figure 1. H & E stain. Mag. 317 ×. Note intranuclear and intracytoplasmic inclusion bodies.

After the virus had been cultivated in human amnion cells for a short time a cytopathogenic ef-

[1] Enders, J. F., and T. C. Peebles, The propagation in tissue culture of cytopathogenic agents from patients with measles. *Proc. Soc. Exp. Biol. & Med.* **86**: 277, 1954.

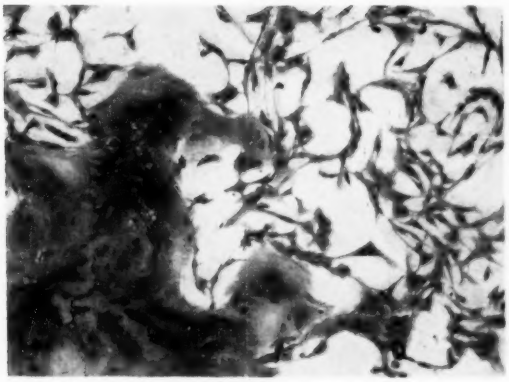

FIG. 3. Spindle cell formation and "giant cell" in culture of human amnion cells produced by measles virus (Edmonston strain). H & E stain. Mag. 78×.

fect very different from the giant cell formations became apparent. Many of the more or less rectangular epithelial cells instead of fusing together became elongated and assumed a spindle or stellate outline. The protoplasmic processes of these affected cells became shaggy or beaded and intranuclear inclusion bodies could be seen in stained preparations. In figure 3 this transformation is shown together with a typical giant cell.

Another variation in the cytopathogenicity of the measles virus has been lately observed in our laboratory.[2] This was demonstrated only after the agent was propagated through a long series of passages in cells of human origin followed by additional passages in living chick embryos. Because the acquisition of this property is of interest from both the biologic and practical points of view, I shall outline the experimental procedures by which it was revealed. In the first place, we consistently failed to cultivate in chick embryos 7 different strains of recently isolated virus. These negative results strongly suggest that measles virus as it exists in nature does not possess the ability to multiply, or at least to multiply actively, in chick embryonic cells. One of these strains was afterwards subcultured 23 times in human kidney cultures and then 26 times in human amnion cultures. At some point during this prolonged period of multiplication under relatively artificial conditions, the capacity to grow in the chick embryo was acquired or, in any event, greatly enhanced. This was demonstrated by the inoculation of virus from the 26th passage into embryonated eggs in which it now grew readily. After the virus had been carried successfully through 6 consecutive chick embryo passages, it was added to chick embryo cells cultivated in test tubes, where, as might be expected, it actively proliferated.[3] In these chick cells, however, during the early passages the virus produced no recognizable cytopathogenic effect. Only in the 4th subculture a proportion of the chick cells were seen to assume the spindle or stellate configuration comparable to that previously noted in infected human amnion cells. Occasionally small multinuclear giant cells were also observed. As subcultures were continued, these transformations became more widespread and prominent.

These observations on the behavior of measles virus emphasize the fact, which has become apparent only during the past few years, that the cytopathogenic properties of viruses may undergo profound modifications in isolated cell systems. It has, of course, long been known that attenuation in the virulence of a virus may occur when it is propagated either *in vitro* or in individuals of a species which it does not naturally infect. This knowledge has been applied in the development of vaccines against rabies, yellow fever, hog cholera, and other diseases of man and animals. Accordingly, from the practical point of view it seems important to determine whether changes in cytopathogenicity observed *in vitro* may reflect alterations in virulence for the natural host. At the moment we are engaged in experiments aimed to discover whether a correlation of this sort may obtain in the case of measles virus that has been adapted to chick embryo tissues.

I shall now present very brief descriptions of the cytopathogenic effects of two other viruses. These effects are similar but not identical with those caused by recently isolated strains of measles virus. The existence of these two viruses, like many others which have been lately discovered was revealed by the changes they produce in tissue cultures. The first agent, which has not yet received a proper scientific baptism, but is called in laboratory parlance the monkey "foamy virus" was described in 1954 by Rustigian.[4] In cultures

[2] Enders, J. F., T. C. Peebles, K. McCarthy, M. Milovanović, A. Mitus, and A. Holloway, Measles virus: a summary of experiments concerned with isolation, properties and behavior. *Amer. Jour. Pub. Health*, **47**: 275, 1957.

[3] Katz, S. L., M. Milovanović, and Enders, J. F., The multiplication and cytopathogenic effect of measles virus in cultures of chick embryo cell. In preparation.

[4] Rustigian, R., P. Johnston, and H. Reihart, Infection of monkey kidney tissue cultures with virus-like agents, *Proc. Soc. Exp. Biol. & Med.* **88**: 8, 1955.

of kidney tissue taken from apparently normal rhesus monkeys and incubated for several days, this worker noted the development of multinuclear giant cells resembling those induced by the measles virus. These changes were then shown to be the results of the outgrowth of an agent that had been latent in the monkey's tissue. In unstained tissue culture material the effects cannot always be distinguished with complete assurance from those produced by measles virus. But in stained preparations, their differentiation is easy, since the foamy virus does not lead to the formation of intranuclear inclusion bodies, although a characteristic rearrangement of the chromatin is evident. The foamy virus, however, causes cytoplasmic injury which is manifested by the development of irregular inclusions as shown in figure 4. It is evident that these bodies are comparable to those appearing in measles giant cells. Foamy viruses are encountered with great frequency in tissues from apparently healthy rhesus and cynomolgus monkeys. Interestingly enough, they have not yet been proved capable of causing overt disease, but appear merely to give rise to latent infections discernible only by the highly artificial procedure of tissue explanation.

The second agent was described in 1956 by Channock [5] and independently by Beale and his associates,[6] in cultures of monkey kidney cells exposed to throat washings from children with the clinical signs of croup. Accordingly, it has been designated as the "CA" or "croup associated" virus. Kibrick, in our laboratory, obtained during the past winter several strains of this agent from cases of infantile bronchiolitis, a pneumonia-like disease. The causal role of CA virus in certain forms of respiratory illness is, therefore, probable, but has not yet been proved. In cultures of epithelial cells derived from monkey kidney or human amnion, the CA virus produces two distinct types of cytopathic change. One consists in the formation of grape-like clusters composed of rounded, highly refractile cells; the other in the development of multinuclear giant cells that may be indistinguishable in unstained material from those induced by measles or foamy viruses. These two quite different effects of the CA virus are shown in figures 5 and 6. Like the foamy virus, the CA agent does not stimulate the formation of intranuclear inclusions, and so in this way can be easily differentiated from measles virus. However, as Kibrick has lately observed, very numerous and large inclusions develop in the cytoplasm of the giant cells as illustrated in figure 6, that in general resemble those induced by the other two agents.

FIG. 4. Multinuclear giant cell formation in a culture of monkey kidney cells infected with monkey foamy virus. H & E stain. Mag. 234 ×. Note large irregular intracytoplasmic inclusion bodies but absence of intranuclear inclusions. Abnormal aggregation of nuclear chromatin is evident, however.

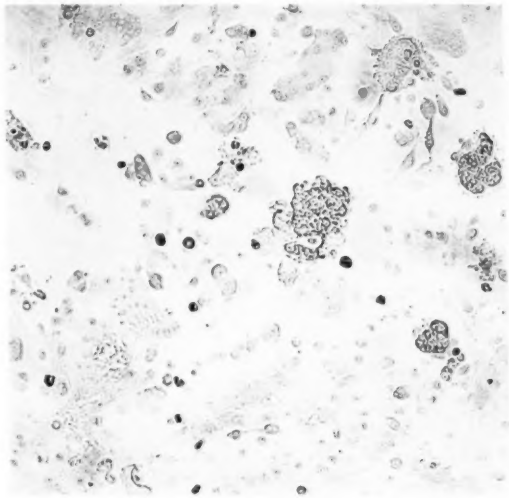

FIG. 5. Effects of CA virus in culture of human amnion cells. Clusters of rounded highly refractile cells (upper right) are seen together with multinuclear giant cells (upper and lower left). Unstained fresh preparation. Mag. 78 ×.

[5] Channock, R. M., Association of a new type of cytopathogenic myxovirus with infantile croup, *Jour. Exp. Med.* **104**: 555, 1956.

[6] Beale, A. J., D. McLeod, W. Stackiw, and A. J. Rhodes, Isolation of cytopathogenic agents from tracheal secretions of cases of acute laryngotracheobronchitis. *Amer. Med. Ass. Jour. Dis. Child.* **93**: 42, 1957.

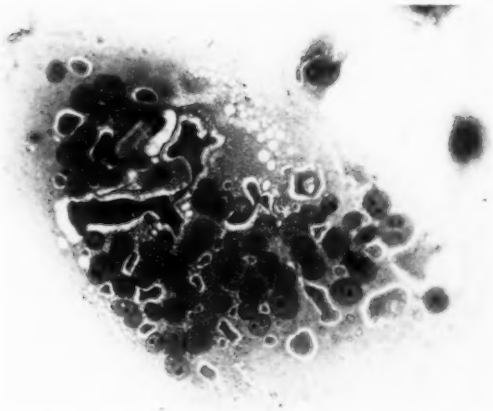

FIG. 6. Multinuclear giant cell in a culture of human amnion cells produced by CA virus. H & E stain. Mag. 468×. Note large irregular cytoplasmic inclusions and normal appearance of nucleus.

GENERAL FEATURES OF VIRAL CYTOPATHOGENICITY

Limitation of time has led me to select only these three examples from a rapidly increasing body of observations on the cytopathogenicity of animal viruses as expressed in cultivated cells.[7] But I hope they will have served to delineate certain common features of this most salient attribute, features I may summarize as follows:

First: cellular responses to injury by viruses tend to be distinctive as compared with those brought about by other noxious agents.

Second: viruses that differ widely in respect to their natural hosts or the diseases they cause may, nevertheless, induce similar cytopathic changes. As in the case of the agents just discussed, similarity in cytopathogenic effect suggests a common or group relationship and so would seem to provide another criterion in addition to the very few now available, upon which a satisfactory classification of viruses may ultimately be based.

Third: while broad similarities in cytopathogenicity may serve to define groups of viruses, it is also evident that distinct differences within a group may exist. Such differences afford a means of differentiating between members of the same group.

Fourth: variations in cytopathogenicity tend to occur. Just as in the living animal alteration in virulence of a virus can be effected by passage from individual to individual or by adaptation to a different species, this tendency may be enhanced by prolonged cultivation of the virus in the same type of cell or by the substitution of one kind of host cell for another.

Fifth: viral multiplication is not inevitably followed by obvious cellular injury or destruction.

FINAL COMMENT

In concluding these fragmentary remarks on viral cytopathogenicity, I would stress our ignorance of its underlying mechanisms. At present we simply do not know how these unique biological entities set in motion what must be an intricate chain of intracellular events that terminate in effects such as I have just described. Various hypotheses have been proposed. To me none are entirely satisfactory, since they do not adequately allow for two of the most outstanding features I have listed, i.e., the diversity of effects produced by different viruses and the occurrence under certain conditions, of viral multiplication without apparent cellular injury. But we are confident that eventually the combined efforts of biochemists and biologists will supply a full account of the factors responsible for viral disease of the cell. Yet I venture to predict that this account will neither be simple nor soon available.

[7] For a review of the literature to 1954 see: Enders, J. F., Cytopathology of virus infections in *Annual Review of Microbiology* **8**: 473–502, 1954. The numerous papers on various aspects of viral cytopathogenicity that have subsequently appeared will be reviewed by J. T. Sylverton in the *Annual Review of Microbiology* **9**, 1956, shortly to be published.

REPEATED PLASMAPHERESIS IN THE SAME PERSON—A RATIONALE FOR MODERN BLOODLETTING

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Department of Pediatrics, School of Medicine, University of Pennsylvania, and the Children's Hospital of Philadelphia

(*Read April 25, 1957*)

THE astonishing acceleration of advances in medicine over the past eighty to ninety years, which has maintained a close parallel to the rapidly accelerating advances in the general field of the biological and of the natural sciences, seems to have all but obliterated the ancient theories of health and disease in man as consisting respectively in the balanced or unbalanced humors of the body. From the earliest writings of the Hippocratic and Cnidian schools of about 400 B.C., as well as from Talmudic and Hindu medicine, even to as late as 1881 when "the last of the great bloodletters," J. B. Bouillaud, a French physiologist, died, a restoration of humoral balance in disease by means of bloodletting was viewed by most as a matter of common sense. In fact, it was only during the last two centuries, the eighteenth and nineteenth, of this entire period of over 2,300 years, that bloodletting—one of the two or three oldest surgical procedures known to man—was seriously questioned as a routine method of treatment.

On the one hand the emphasis on anatomical localization culminating in the cellular pathology of Virchow together with the work of Claude Bernard and others began to erect boundaries in the humoral wilderness of Galen in which medicine had wandered aimlessly for so many centuries. On the other hand, the studies of the statisticians, though often contradicting one another, combined with a general sceptical trend, tended to throw doubt on traditional practices. As a result, the value and the rationale of bloodletting were questioned with increasing insistence, and excessive bloodletting was condemned as harmful by ever greater numbers of physicians.

The obvious harm of bloodletting in excessive amounts, such as those "let" by the disciples of Benjamin Rush which apparently caused the death of George Washington, rapidly tended to obscure any possible rationale for, as well as any possible benefit from, this venerable procedure.

While fully recognizing the irrational humoral concept of disease and the grave dangers inherent in bloodletting by men like Rush who thought the body held at least double the amount of blood actually present, the thesis outlined in part of this report still holds that repeated removal in the same person of "humors" by the more adequate means of bloodletting now available may be life-saving to that individual. These more adequate means of bloodletting recently developed are two: (1) the exchange transfusion, and (2) repeated plasmapheresis in the same person. The first of these methods has limited usefulness while the latter method, developed in our laboratories as a valuable routine procedure, has rather dramatically opened opportunities not only for treatment of the diseased individual, but particularly for the stockpiling of various important biological constituents of the blood plasma, or even of the whole blood, which were hitherto not available.

Two names of former members of this society have already been mentioned above, George Washington and Benjamin Rush. To another former member, Edwin Cohn, with whom one of the authors (Stokes) closely collaborated over many years in the development and use of human serum albumin and gamma globulin, credit is due for the development of the fractionating centrifuge by which our desired objective—repeated plasmapheresis in the same person over periods at least as long as one year—could be attained. While we have participated in the detailed perfecting of equipment for the centrifuge and though the concepts for the methods of repeated plasmapheresis have been our own, Dr. Cohn's and the Arthur D. Little Company's development of the basic design, whereby sterile separation of the unchanged biological fractions could be quickly obtained, has been essential for the success of these studies.

Essentially the present studies are related to the ancient art of bloodletting in only one important respect, namely that of ridding the body of its "poisonous" or toxic materials circulating in

the blood. Bloodletting as practiced over the centuries permitted no replacement of either formed or liquid constituents of the blood, entirely aside from the later art of returning the donor's own red cells and of replacing his own "toxic" plasma by normal "non-toxic" plasma.

If the term bloodletting may be used more broadly and in the modern sense of taking blood from the donor for purposes of storage and future use, then indeed bloodletting may be considered to have a broad and sound rationale far surpassing any uses dreamed of by Benjamin Rush and his predecessors.

In another sense, however, the present method of storage of human blood products assumes an added significance in the rationale of bloodletting, namely that the ancient art and the present studies have depended upon repeated bleedings over considerable periods of time—the older method, however, solely for purposes of relieving the body, the present newer method for purposes of storage of valuable products as well as for purposes of relieving the body of circulating "poisons."

In the obtaining of blood, plasmapheresis may be defined as the immediate return to the donor of his red blood cells with the retention of his plasma.

Repeated plasmapheresis in animals, particularly in dogs where the rapid return of the red cells and sterility have not been as important, has not been an uncommon procedure in recent experimental studies, as illustrated chiefly by the work of George Whipple (1938). This work gave considerable assurance for the safety of using a similar method in man.

In the present studies repeated plasmapheresis with immediate return of the red blood cells within approximately twenty-one minutes and the retention of plasma has been carried out in man over the period of at least one year at biweekly intervals. In all of this work an ADL Cohn Fractionator has been used as shown in the accompanying photograph (fig. 1). The non-wettable surfaces of plastic equipment, of a siliconed needle, and of the siliconed centrifuge bowl all are autoclaved in a single unit which primarily accounts for the maintenance of sterility in over nine-hundred plasmaphereses. After the centrifuge is running at proper speed, saline is run from the bottle which flushes the air out of the tubing and washes the resin. The saline tubing is then clamped off and the siliconed needle inserted into the antecubital vein of the donor. The onrushing blood pushes the saline into a side waste bag which

FIG. 1. ADL Cohn fractionator. The return of red cells to donor on left while blood is withdrawn from donor on right.

is clamped when the blood has cleared the resin. The resin exchanges sodium for the calcium of the blood and renders the blood non-coagulable. The blood then flows upwards in a tubing through the center of the centrifuge bowl which has the shape of a truncated cone and is spinning at about 3800 r.p.m. The blood spreads out from the top of the bowl along the sloping sides, the red cells hugging the siliconed surface and pushing the lighter plasma towards the center. As the inner edge of the plasma reaches holes provided on the bottom and towards the center of the bowl (fig. 2) it passes downward through the holes and is thrown outward against a baffle provided with holes through which the plasma enters a plastic bag. As the amount of blood increases in the bowl the plasma is continuously skimmed off in this manner until the edge of the packed red cells closely approaches the holes in the bottom of the bowl. This can be accurately determined from experience with the length of time required for the packed cells to fill the bowl up to the holes, i.e. about nine minutes. A brake is then applied which stops the bowl in a fraction of a second and the red cells pass through the same holes which

FIG. 2. The complete cartridge unit after separation of red cells and plasma. Before bleeding complete unit as shown is sterilized by autoclaving.

skimmed the plasma, but since the centrifuge is stopped they fall directly downward into a second plastic bag provided for the cells. Upon the stopping of the centrifuge and during the drainage of a small amount of remaining whole blood into the red cell bag the saline is run through the Y-tube and through the donor's needle to keep it patent and to restore to an extent the volume of fluid lost (fig. 1). A dielectric seal is used to close off the tubing of each of the bags (fig. 2) and the middle of each seal is cut by scissors, thus maintaining sterility. The plasma bag is removed for storage and the red cell bag is elevated and squeezed by a pressure cuff for return of the cells to the donor through a female adapter on the tubing of the red cell bag which is fitted into a male adapter on the tubing slightly distal to the donor's needle. The red cells with a small admixture of plasma and with a hematocrit of approximately sixty-five are returned within about nine minutes to the donor, thus resulting in an over-all time for the plasmapheresis of about twenty-one minutes. Four to five complete separations can be performed in one hour since, as shown in the photograph, the second donor can be started with the second centrifuge bowl while the first donor's red cells are being returned.

Twenty-three healthy donors have received biweekly plasmapheresis, four of whom had been previously hyperimmunized with commercial pertussis vaccine. Extensive blood studies were carried out at each visit. The tests for evidences of safety of the procedure at the time of each donation were a red cell count, hemoglobin, hematocrit, white cell and differential count, platelet count, sedimentation rate, plasma nitrogen content, paper electrophoretic pattern and prothrombin clotting time. Other determinations carried out less regularly or only at such intervals as seemed essential, were reticulocyte count, red cell fragility, and bone marrow examination. In table 1 are shown the four groups of twenty-three donors and in table 2 the determinations for many of these values on a single donor through the year during which he had 500 milliliters of blood removed with retention of about 200 milliliters of plasma at biweekly intervals. A total of 5,200 milliliters was received from each donor over the period of one year. This is almost twice his original volume of plasma. Since little if any change was noted in successive determinations only the six-week intervals are shown. There has been no evidence of change in the normal values over the year of study, nor has the bone marrow of the six donors examined shown any detectable change from normal.

TABLE 1

GROUPING OF DONORS

Group	No. of donors	Vaccine injected
I*	7	H. pertussis
II**	4	H. pertussis
III†	6	Mumps
IV	6	Control

* Three injections of commercial H. pertussis vaccine at biweekly intervals between the sixth and tenth weeks.

** This group of men had been used for several years in the preparation of pertussis hyperimmune serum.

† Three injections of commercial mumps vaccine at biweekly intervals between the sixth and tenth weeks.

The method of repeated biweekly plasmapheresis in man was thus shown to be safe and also practicable. The red cell has always been a waste product except in whole blood transfusions. Whereas previously when the red cells were not

TABLE 2

EFFECT OF BIWEEKLY PLASMAPHERESIS ON THE SAME DONOR OVER A PERIOD OF FIFTY-TWO WEEKS

	Weeks									
	0	6	12	18	24	30	36	42	48	52
RBC $\times 10^6$	4.8	4.6	5.0	4.6	5.0	5.6	4.9	5.1	5.3	5.2
Hb	15.1	15.8	15.5	14.6	15.2	15.8	15.7	16.3	15.8	15.9
Hct.	41	41	41	43	46	46	45	46	44	45
WBC $\times 10^3$	8.6	10.8	7.1	5.2	9.0	7.5	5.4	6.6	5.1	9.2
Platelet $\times 10^3$	224	210	172	184	176	288	206	220	190	220
P–T*	22 01	18 28	16 10	17 07	17 33	19 28	21 30	16 59	17 28	17 07
N%	1.09	1.03	1.03	1.04	1.05	0.96	0.98	1.01	1.04	1.02

* P–T = Time for complete plasmapheresis; first two numbers are minutes and the second two numbers are seconds; e.g., 22 01 = 22 minutes and 1 second.

returned, the safe limit of whole blood donations for a single donor was about four to five times yearly, it was clear that a method was now available which permitted the biweekly, or even weekly, obtaining of sterile plasma from healthy donors.

In both animals and men their restorative capacity after plasma loss is astonishingly great, while such is not the case after the loss of the cellular elements of the blood. Because his red cells are returned to him immediately the donor rarely experiences the faintness and other disagreeable sensations at times accompanying sudden changes in blood volume and in the oxygen carrying capacity of the blood resulting from a rapid loss of red cells. These phenomena frequently have been deterrents to the repeated donations of whole blood. The psychological value of the return of his own red cells to the donor also is considerable.

Because of its safe and practicable nature this new method of repeated plasmapheresis in the same person has had an increasing number of applications. The three which have seemed most urgent and on two of which we have considerable data are:

1. The stockpiling of single sterile plasmas in plastic bags at room temperatures over a period of at least six months.
2. The obtaining of considerable quantities of hyperimmune sera from donors immunized or hyperimmunized to mumps and pertussis antigens.
3. The removal of materials from the circulating blood which may endanger life and their replacement by normal or more normal plasma constituents.

1. The basis for the stockpiling of single plasmas has already been outlined. Only certain details remain to be filled in. The single plastic bag containing the plasma, as mentioned above, is inclosed in an additional plastic container which prevents the escape of water vapor (fig. 3). Holding plasmas at room temperature over six months permits relative assurance of their sterility with respect to serum hepatitis virus, although the

FIG. 3. Plastic bag with plasma enclosed in additional plastic container to prevent water transmission at room temperature.

TABLE 3

PERTUSSIS AGGLUTININ TITERS IMMUNE PERTUSSIS GROUP I

Donor	Weeks								
	0	6	12	18	24	30	36	42	48
A. A.	40	40	640	1280	1280	640	640	1280	1280
P. C.	0	0	160	160	320	160	160	320	160
L. P.	0	0	40	80	20	20	80	80	40
L. M.	80	80	1280	1280	640	1280	1280	1280	1280
F. V. C.	0	20	160	160	80	160	160	160	160
L. R.	0	20	80	80	160	80	80	80	80
R. U.	40	40	1280	1280	640	640	640	1280	640

fact that they are single plasmas indicates a degree of safety from the hepatitis virus similar to that of whole blood from donors of a blood bank which is under continuous scrutiny and follow-up. An attached plastic tubing several inches in length to which the plasma in the bag has access permits repeated testing for sterility without entering the bag itself, since when a sterility test is required a distal portion of the attached tubing is closed off by a dielectric seal and the trapped plasma distal to the seal can then be tested without the possibility of contaminating the entire unit. If desired this may be repeated until the length of tubing is exhausted; although a single test prior to use should usually be sufficient. The plasma unit is stored for instant use with an enclosed sterile needle attached to the outlet tubing.

2. The objectives for obtaining large quantities of immune or hyperimmune human sera are obvious. Severe reactions and not infrequently death are constant dangers in the use of immune horse serum preparations. In addition the antibodies produced against horse serum by the first injection in man can make a second injection ineffective, as has been found in tetanus. It has also been shown by studies of Ipsen that a small number of units of human diphtheria antitoxin could be as effective in the therapy of severe diphtheria as a many times greater number of units of horse diphtheria antitoxin. Table 3 shows the results of repeated plasmapheresis in donors immunized to pertussis antigen. An interesting corollary finding has been that repeated injections of antigen, which ordinarily are necessary for assuring the continued elevation of antibodies, become unnecessary when biweekly plasmapheresis is carried out. This indicates a mechanism for sustained production of antibodies which hitherto has not been described in man. An elevation of the plasma nitrogen, which is a measure of protein, for several weeks following the cessation of the regular plasmapheresis is in agreement with the presence of such a sustained production. The usual drop in antibody titer if plasmapheresis is not used is shown in table 4. This stimulation to antibody formation as a result of regular removal of plasma is receiving further study and will be reported more extensively elsewhere.

3. What may be accomplished by the use of plasmapheresis in ridding the body of materials which may endanger life is greater in prospect than in retrospect. However, from data already available and from the present recognized value of exchange transfusion this use of the method would be highly successful. One of the authors (Smolens) in conjunction with members of the staff of the Hospital of the University of Pennsylvania has used the method with ion exchange resin to reduce the potassium in the plasma of man, and from these data it would appear that the plasmapheresis method with return of resin-treated whole blood or normal plasma may be simpler and more effective than the "artificial kidney." It could be highly effective in salicylate, boric acid, phenobarbital, and other poisonings which have responded to exchange transfusion and is well worth trial in other acute or even chronic diseases. The safety and simplicity of the method in over nine hundred sterile plasmaphereses already completed

TABLE 4

PERTUSSIS AGGLUTININ TITERS

Weeks	Final serum dilution								
	20*	40	80	160	320	640	1280	2560	5120
0	4	4	4	4	3	3	2	1	0
8	4	4	4	3	1	0	0	0	0

* Reciprocal of dilution.

may be said to have assured its broad usefulness in eliminating certain materials which are harmful to the body and of which the body cannot rapidly rid itself.

Certain additional uses of plasmapheresis in man may prove to be even more important than the specific ones here briefly outlined.

The albumin and globulin fractions, including salt free albumin and hyperimmune globulins, antihemophilic globulin for hemophilia, platelet preparations as demonstrated by Tullis, rare antibody preparations such as Rh antibodies, or fleeting antibodies as possibly in varicella or rubella, all may be studied and utilized more fully by this method. Approximately one hundred platelet units have been prepared in our laboratories, about fifty of which have been used clinically with excellent responses. Table 5 shows the relative numbers of donors needed to obtain five million plasma units per years. For the standard methods of one or five bleedings per year, one to five million donors are required, whereas with plasmapheresis only one hundred thousand donors are necessary.

TABLE 5

THE NUMBER OF DONORS REQUIRED IN ORDER TO OBTAIN FIVE MILLION PLASMA UNITS (200 MILLILITERS) PER YEAR

Method	Number of bleedings	Number of donors
Standard	1	5,000,000
Standard	5	1,000,000
Plasmapheresis	50	100,000

SUMMARY

The art of bloodletting as conducted for over two thousand years may be said to be attaining in the present development of repeated plasmapheresis in man some measure of its original objectives —relieving man of harmful circulating products in the blood.

In the repeated bleeding and immediate return of red cells, if one also includes normal and specially prepared donors as a modern form of bloodletting which is safe and practicable, an important rationale for such repeated bleeding becomes apparent. In the storage and fractionation of plasma products the method described greatly conserves the number of donors needed, maintains sterility, aids the stockpiling of human plasma, and the collection of hyperimmune globulin from specifically immunized or convalescent donors. Thus for the first time a method is available which provides for real economy in the use of whole blood.

The increasing uses and value of the method and of the products obtainable are extending into an ever-broadening area for study.

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INFLUENCE—PAST AND PRESENT—OF FRANÇOIS-ANDRÉ MICHAUX ON FORESTRY AND FOREST RESEARCH IN AMERICA *

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IT IS necessary briefly to recall a bit of the historical setting of the times during which on three occasions Michaux viewed the American scene at the end of the eighteenth and beginning of the nineteenth centuries.

When European settlers began to arrive, temperate North America from the Atlantic to the prairies beyond the Mississippi was with only minor exceptions clothed with continuous, superb, and diversified forest. For the newcomers the forests provided an incredible abundance of choice woods for housing, fuel, fencing, and the arts—the last then still largely activities in wood and forest products. But the primary problem of the settlers was food; and the light requirements of photosynthesis being what they are, food production from cultivated crops necessitated clearing. Thus began what might be termed the dominant activity of the settler—clearing—converting virgin forest land to clear land. For the untold billions of feet of even the finest wood felled in the process, there was by and large no market and no use. It was therefore largely consigned to the flames.

We are tempted to pass harsh judgment on this unparalleled forest and wood destruction, as did some from Europe at the time, where even then forests were the object of careful management. But to the settler in the primeval forest the trees, beyond supplying wood for his immediate needs, were in a real sense formidable weeds standing in the way of agriculture. Professor Chinard, in his admirable paper on the early history of forestry in America,[1] quotes from an apparently unpublished manuscript by Benjamin Vaughan, a thoughtful observer of the contemporary scene at the end of the eighteenth and beginning of the nineteenth centuries. After describing in detail the destructive clearing methods in extreme southwestern Maine, the least remote part of the state, Vaughan remarks: "A foreigner may judge the system improvident; but were he transported to the scene of it, he would in a short time cease to wonder."

Indeed, the hardy activity of the clearer of the forest became a symbol of virtue. At least at the time of their adoption, it may be assumed that great seals attempt a more or less idealized portrayal of laudable qualities of the territory or state they represent. Aside from such ideals as justice, freedom, etc., the great seals of the several states of the United States emphasize, among other pursuits, commerce, mining, and above all agriculture, with the plow and grain shock—sometimes accompanied by tree stumps—the most frequently recurring elements. The great seal of the state of Indiana is especially interesting in this connection. Comprising substantial parts of the drainage basins of the lower Ohio, Wabash, and White Rivers, the forests of the state constituted some of the most magnificent deciduous timber of the continent. Impressive is the number of hardwood species recorded in Sargent's *Sylva* as reaching their greatest dimensions in these valleys. Such were the forests which fell under the ax of the pioneer Indiana settlers.

Used as early as 1802 and throughout the territorial period, and reaffirmed in 1816 when the territory gained statehood, the great seal of Indiana depicts the following scene: Over the mountains to the east, i.e., the Alleghenies, the sun of enlightenment is rising. The central element is a sturdy pioneer, representing civilization subduing the wilderness, in the act of hewing down a great tree at the edge of the primeval forest. And in the foreground a woodland bison, symbolizing primitive life, is fleeing westward before the advancing civilization.[2]

* Presented at the Centennial Observation of the death of François-André Michaux at the Colloque International No. 63, Paris, September 11–14, 1956. The whole report of this colloquium is found in the book, *Les Botanistes Français en Amérique du Nord*, published by Centre National de la Recherche Scientifique, 360 pp., 34 pl., Paris, 1957.

[1] Chinard, Gilbert, The American Philosophical Society and the early history of forestry in America, *Proc. Amer. Philos. Soc.* **89**: 444–488, 1945.

[2] Dunn, J. P., *Indiana and Indianans, a history of aboriginal and territorial Indiana and a century of statehood*, Chicago and New York, The American Historical Society, 1919. See Vol. 1, pp. 378–379.

It is tempting to reflect on what elements of design would be considered worthy of the State's present high aspirations were it to adopt a great seal today. Surely it would not proudly proclaim forest and game destruction as virtues. On the contrary, it might now portray tree planting and the return of wild life to hospitable forest haunts; certainly, the state is working assiduously toward those goals. However that may be, this bit of symbolism, in the manner of a miniature, may perhaps dramatize how we in the United States have come—of necessity of course—full cycle in our attitude towards the forests.

Meanwhile, conditions in the East were changing. In the older cities and towns even wood for fuel was becoming scarce and dear. To supply timber for a growing population and expanding industry and commerce, sources more and more remote, notably along waterways, were being tapped. Gradually forests were becoming less impediments to progress and more essentials to current economic life. From this stirring of concern in populous centers on the Atlantic fringe to the raw battle with the forests on the frontiers beyond the Alleghenies and Appalachians, the vast expanse of territory must have presented at one and the same time a veritable spectrum of conditions and of attitudes of the populations in their relation to the forests.

Of this spectrum François-André Michaux gained first-hand knowledge in the course of three extended visits to America. Every general region east of the Mississippi except the extreme northwest and far southwest came under direct observation. What sets him apart from earlier botanists visiting North America, including in some degree his father, is that his interest was forest botany and forestry rather than strict systematic and floristic botany, important though his contributions to the latter fields are. Soil and moisture requirements of forest species, growth habits and rate, succession as observed in abandoned clearings, etc., engage his attention. To the future American forester he envisions, but who was not to come upon the scene for nearly a century, he recommends encouragement of certain species and curtailment of others.

And certainly no less inquiring is he concerning the characteristics and uses of wood, bark, etc., of the various tree species. From shipyards to tanneries, from the makers of furniture to coopers and dyers, etc., he learns from artisans and others what woods are used and preferred and why. Weighing all the information acquired, and in the light of his wide knowledge of European species and conditions, he reaches his conclusion whether on balance a given species would be a desirable addition in Europe. It is to be remembered that the missions of father and son centered around the introduction into France of promising species from America, notably for naval construction. That his interest is forestry and forest botany rather than ornamental horticulture, is thinly disguised. Again and again American species are dismissed almost in offhand manner as potentially useful in Europe merely for ornamentation.

Familiar with the dire consequences in France of neglect of forest regulation and management, how did Michaux react to the headlong destruction and waste in the New World and the absence of thought for the future? He might have been pardoned for emotional protests over what he saw. But so far as his two principal works [3] reveal, he maintained a calm, restrained, and scientific poise in his comments on what he saw and learned. He was an advocate of forest conservation, not a propagandist. A few examples will have to suffice, all taken from the *Sylva*. In the account of eastern white pine (*Pinus strobus*), he writes:

> Soon after the establishment of the colonies, England became sensible to the value of this resource and solicitous for its preservation. In 1711 and 1721, severe ordinances were enacted, prohibiting the cutting of any trees proper for masts on the possessions of the crown. The order comprised the vast countries bounded on the south by New Jersey and on the north by the upper limit of Nova Scotia. I am unable to say with what degree of rigor it was enforced before the American Revolution; but for a space of 600 miles, from Philadelphia to a distance beyond Boston, I did not observe a single stock of white pine large enough for the mast of a vessel of 600 tons.

From the account of bald cypress (*Taxodium distichum*) is quoted:

> It would be unavailing to recommend the preservation and multiplication of the cypress in the maritime districts of the Carolinas and Georgia; though for an extent of more than 900 miles they have neither stone nor slate for building, it becomes daily more profitable for the increasing population to convert the marshes into rice grounds, which afford a sure subsistence and swell the mass of exported produce. Instead of wood, the houses will be constructed

[3] Michaux, F.-A., *Travels to the west of the Alleghany Mountains*, Translated from the French, London, 1805.—*The North American Sylva*. Translated from the French, Philadelphia, 1857.

of bricks, which is already beginning to be done, and covered with slate imported from the northern states or from Europe. It is highly probable that in less than two centuries the cypress will disappear from the southern states.

The waste of wood in supplying tanbark appalled him, magnificent trunks of hemlock or even of white oak often left to rot on the ground after removal of the bark. He is deeply concerned over the enormous fuel consumption in potteries, tile and brick kilns, distilleries, crude evaporators at salt springs, sugar refineries, including maple sugar, etc., and clearly sees the day of reckoning coming. In some cases he suggests improvements to reduce fuel requirements. On the other hand, in discussing American chestnut (*Castanea dentata*), he commends iron furnaces in Pennsylvania which maintain a steady supply of chestnut for the excellent charcoal it yielded by a coppice system in a sixteen-year rotation—a system he recommends to forges in Virginia and the Carolinas.

On occasion, however, a practice is so wanton as to ruffle Michaux' usual calm. In discussing the cabbage palm (*Sabal palmetto*) he observes:

> The base of the undisclosed bundles of leaves is white, compact, and tender; it is eaten with oil and vinegar, and resembles the artichoke and the cabbage in taste, whence is derived the name of cabbage-tree. But to destroy a vegetable which has been a century in growing, to obtain three or four ounces of a substance neither richly nutritious nor peculiarly agreeable to the palate, would be pardonable only in a desert destined to remain uninhabited for ages. With similar prodigality of the works of nature, the first settlers of Kentucky killed the buffalo, an animal weighing 1200 to 1500 pounds, for the pleasure of eating its tongue, and abandoned the carcass to the beasts of the wilderness.

Surrounded by seemingly inexhaustible forests except in some parts of the east, Michaux at no time is misled as to the future. Here we may quote from his account of the red-flowering maple (*Acer rubrum*), in which incidentally is revealed his sound analysis of the merits or demerits of individual species:

> When the period arrives that it is necessary in the United States, as in Europe, to renew the forests, or to preserve those which have escaped destruction, the American forester will find among the oaks, the walnuts, and the ashes, many species more deserving of his care. The sugar maple also will be preferred, which grows on uplands, and possesses in a superior degree all the good properties of the other [i.e., red-flowering maple]. From these considerations, the red-flowering maple appears to have no pretensions to a place in European forests.

Reference should be made also to Michaux' habitat and ecological observations, the quotation being from the account of the silver maple (*Acer saccharinum*):

> . . . the white maple is found on the banks of such rivers only as have limpid waters and gravelly bed, and never in forests where the soil is black and miry. These situations, on the contrary, are so well adapted to the red maple that they are frequently occupied by it exclusively. Hence the last mentioned species is common in the lower parts of the Carolinas and of Georgia; for as soon as the rivers, in descending from the mountains toward the ocean, reach the low country they begin to be bordered by miry swamps covered with cypress, black gum, large tupelo, etc.

It is difficult to say what the direct effect of the *Sylva* was in initiating the conservation movement in America, fundamental though it was for the development of scientific forestry. It is the solid work of a dendrologist and forester, not of a propagandist. Perhaps Michaux himself was under no illusions regarding the influence it would exert, aware that the "imperious necessity" of which he speaks in Europe was not as yet plain for all to see in America. In any event, Michaux provided other strings to his bow. The legacy he left to the American Philosophical Society, of which he became a member in 1809, was, to be sure, an expression of gratitude for the hospitality and help he and his father had received in America. But it was more than that. The other objective, as stated in the covering will, was to contribute to the extension of agriculture and more especially of sylviculture in the United States. And to the "Society of Arts and Agriculture of the State of Massachusetts, Boston," of which he states he is also a member, was bequeathed a sum one third smaller but with the same purposes. Not being familiar with the history of the latter legacy, I shall confine myself to a brief review of the influence of the former.

Though the income from the bequest did not begin to come into the hands of the American Philosophical Society until after the death of Madame Michaux in 1870, and the first disbursement was not authorized until late 1871, the Society in 1869 already had a Committee on the Michaux Legacy. Its chairman was Eli Kirk Price, chairman also of the Committee on Trees and Nurseries of Fairmount Park in Philadelphia, then in its beginnings and soon to be the site of the Centennial Exposition of 1876.

On September 17, 1869, the Society adopted the following resolution [4]:

That the Committee on the Michaux Legacy be authorized to make inquiry whether the trust under the will of André François Michaux may not be executed in whole or in part within the Fairmount Park; with authority to communicate with the Commissioners of the Park upon the subject; and to make report to the Society.

Under date of March 12, 1870, the Park Commissioners addressed the following communication to the Society [5]:

Whereas, The American Philosophical Society has made a communication to this Commission, proposing that . . . André François Michaux, who travelled long in this country, and described our Oaks and forest trees, in a work of great merit and splendor, should have his name, and that of his father (who had, by like travel and study, rendered service to science), honored in the Fairmount Park, in a manner to be a memorial to their devotions, and to promote the objects which had occupied their lives, and has proposed, after the death of the widow of André François Michaux, to devote the interest or income of six thousand dollars bequeathed by him to said Society, to be expended in execution of the trust of his will in the said Park, for the purpose following. Therefore,

Resolved, that there shall be a grove of Oaks in the Fairmount Park forever to bear the name of "The Michaux Grove," in which, if practicable, shall grow two oaks of every kind that will endure the climate.

Resolved, that any surplus of revenue received by the Commission from the Michaux Fund, after satisfying the requirements of the preceding resolution, shall be devoted to the cultivation of Oaks of every variety capable of cultivation in our climate, in the Park nursery, which Oaks, to the extent of two of each kind cultivated, be hereafter distributed to other Public Parks in the United States, under proper regulations to be hereafter prescribed.

To the terms of this resolution the Society agreed; and in May, 1870, chairman Price reported [6] to the Society that a site for the Michaux Oak Grove had been selected. Also, that it already contained seventeen kinds of oaks and that "all other kinds that will live in this climate will be imported from France."

In 1873 about $300 income from the Michaux Fund was spent in setting out some 100 foreign species, varieties, and sports of oaks, purchased abroad by J. C. Cresson, Chief Engineer of Fairmount Park Commissioners, after consulting Sir Joseph Dalton Hooker at Kew, visiting English nurseries, consulting catalogues, etc. In 1876 the Society consented to a proposal by the Park that after providing for the Michaux Grove and for cultivating other oaks as previously agreed, the residue of the income of the legacy paid to the Park may be applied to the purchase of other trees. And that such other trees as well as oaks may be distributed to parks, city squares, institutions of learning and charity, etc. And a little later Price is able to report that the park nurseries contain nearly 80,000 trees and shrubs, of which about 30,000 are ready to plant out. An enumeration lists among others 5,000 American arborvitae, 2,000 European beech, and 800 European oaks. Of the oaks, Price reports, a few were ordered planted in the campus of the University of Pennsylvania. But, as appears later, this considerable nursery stock was essentially for planting the new 3,000-acre Fairmount Park, the Centennial Exposition having just concluded.

It will be unnecessary to pursue further the history of this first undertaking supported by the Michaux Fund. It falls in the period when the states, one after another, were instituting an annual arbor day, and when in other ways tree planting in the minds of many seemed one way out of the growing forest problem. Certain it is that the developments in the Park honored the Michaux, and that Fairmount Park was greatly beautified; also that this essentially horticultural development had botanical overtones by virtue of the variety of native and exotic ligneous plants acquired and displayed to the public. But as a means of advancing sylviculture or stimulating an effective realistic conservation movement, its effect was probably at best slight.

But if the contribution to forestry of this first effort was not impressive, there can be no doubt of the lasting and highly significant influence of the next major undertaking. Early in 1877 the Michaux Legacy Committee, through its chairman Eli Kirk Price, recommended [7] to the Society that an appropriation be made out of the income of the Michaux legacy to defray the expenses of a course of lectures "contributing to the extension and progress of sylviculture," to be delivered by Dr. Joseph T. Rothrock, Professor of Botany at the University of Pennsylvania in Philadelphia. The recommendation was adopted and Rothrock was invited to submit a syllabus for the course of

[4] *Proc. Amer. Philos. Soc.* **11**: 193–194, 1869–1870.
[5] *Proc. Amer. Philos. Soc.* **11**: 312–313, 1869–1870.
[6] *Proc. Amer. Philos. Soc.* **11**: 440–441, 1869–1870.
[7] *Proc. Amer. Philos. Soc.* **16**: 667, 1876–1877.

fourteen lectures to be given in Fairmount Park at Horticultural Hall, the latter appropriately enough situated in the Michaux Grove of oaks. Thus began a sustained joint effort of the Michaux Fund and Dr. Rothrock which continued from 1877 to 1894.

In this first course, as revealed by the syllabus, basic forestry subjects predominated with four or five on supporting botanical topics, including plant geography, succession, structure, reproduction, and physiology—altogether a substantial fare for a general audience. Despite adverstising in the newspapers and by handbills, the audience at the first lecture, so Rothrock tells us, consisted of the "janitor and two others." As the course progressed attendance increased moderately. But Rothrock apparently realized that popular interest in forestry, at the time slight, would be more rapidly generated if his approach were through subjects with currently greater popular appeal; for, in his words, "he needed some ears besides his own." In any event, the subjects for some years were less overtly forestry, and more directed toward cultivating the interests of his audiences through a variety of topics, chiefly botanical and changing from year to year.

By 1881 the lectures were drawing audiences up to 400, including notably many teachers. But never did Rothrock loose sight of his goal. Probably no annual course failed to strike some blows for the cause of forestry. And as time went on and the tide began to turn, the forestry ingredient increased. And in the final and fifteenth annual course in 1894, not only was the entire series devoted to forestry but it was specifically directed toward a program for the Commonwealth of Pennsylvania.

The lectures were never assembled and printed. With approval of the Society, some in later courses were abstracted by Rothrock in *Forest Leaves*. The latter, together with news reports in contemporary newspapers and topical lists in the *Proceedings* of the Society, are apparently our only published sources of the contents of what became widely known as the Michaux Lectures on Forestry under the auspices of the American Philosophical Society.

Certain it is that in this joint enterprise the means, provided by the Michaux legacy, and the man were ideally joined. A glance at what meanwhile transpired in Pennsylvania at large discloses the cumulative effect of this memorable partnership. The lectures attracted the press, which increasingly came to Rothrock's support in the forestry issue and helped powerfully to develop statewide favorable public opinion. In 1886 was founded the Pennsylvania Forestry Association, with a membership which soon represented many parts of the state. Its publication—*Forest Leaves*—speedily became influential and multiplied the influence of Rothrock's pen. Its prestige may be judged from the fact that for a time it served as the official organ also of the American Forestry Congress and of its successor, the American Forestry Association—early organized efforts on the national scene. And finally in 1893 the law establishing the Pennsylvania Forestry Commission was enacted.

At the core of all these developments was Rothrock; his appointment as the botanist member of the Commission (Col. A. Harvey Tyson the engineer member) on June 8, 1893, by Governor Pattison, himself a friend of conservation, was therefore logical, non-political, and auspicious. Anticipating the need for basic information regarding the forest situation in Pennsylvania, Rothrock addressed a letter to the Michaux Legacy Committee which the latter presented to the Society at its January 20, 1893, meeting.[8] It read in part as follows:

> I most respectfully request that the Society will appropriate from the income of the Michaux Fund the sum of $300 . . ., to aid in defraying the expenses of collecting facts and statistics bearing upon the relation of our forests to the Commonwealth. . . . Notwithstanding the general interest in the subject of forestry, there is an utter absence of the data required for any proper presentation of the important questions involved. It is thought that no use more immediately helpful and more in accord with the wishes of the testator could be made of the money.

The Michaux Legacy had made possible the sustained educational effort that was about to result in the creation of the Pennsylvania Forestry Commission. Now it was enlisted also in gathering the facts which such a commission needed as a basis for its report to the legislature and eventual forestry legislative proposals.

With the creation of the Commission, Rothrock threw himself into the formidable task of securing the most necessary information for the Commission's report, due for presentation to the legislature within two years. The resulting report,[9] all

[8] *Proc. Amer. Philos. Soc.* **31**: 7–8, 1893.

[9] Annual Report of the Pennsylvania Department of Agriculture for 1895. Part II. Division of Forestry. Comprising Report of Pennsylvania Forestry Commission, Approved by Act of Legislature, May 23, 1893.

but 18 of its 348 pages from Rothrock's pen, was memorable. Forest statistics, topographic and watershed data, and other pertinent information are presented for each county, together with an account of the principal forest species of the state. Included also are statistics on tax delinquincy on especially lumbered lands, and on the appalling annual toll of fires. Special attention is devoted to suitable areas for state forest preserves in the non-agricultural headwaters of streams to serve the dual purpose of supplying timber for the future and to contribute to flood control while at the same time tending to stabilize stream flow.

Here is a document refreshingly factual and scientific, and free of sentimentality, the latter rarely serving the cause of sound conservation. After sixty years it still is profitable reading. Indeed, it is a landmark in the emergence of effective forestry in America. Unlike many proposals of the time, it recognized clearly that the first crying need was protection and sound management, including notably fire control, of the forests yet remaining. It is to be remembered that at the time replanting, even extensive planting of the treeless prairies, was being advocated as a primary approach to the ominous forestry problem. But Rothrock, like B. E. Fernow, put first things first —the proper handling of what had escaped destruction.

Rothrock acquired botanical training under Asa Gray at Harvard University and experience as a field botanist especially when as surgeon and botanist he accompanied the Wheeler expeditions of 1873–1875 west of the 100th meridian. His early interest in forestry was intensified when in 1880, i.e., several years after he began the Michaux lectures, he spent nine months at the University of Strassburg studying under Anton DeBary. Here he became profoundly impressed by the German policies of forest conservation. He returned with a determination that amounted to dedication to arouse public opinion to the critical need of forest conservation in America and especially in Pennsylvania. With sustained support from the Michaux Fund, the results have already been sketched. It remains only to add that, following submission of the Forestry Commission's report, a Department of Forestry was established in the State Department of Agriculture, with Rothrock as its first Commissioner. And finally in 1901 a full-fledged Commonwealth Department of Forestry was created, Rothrock again becoming its first head.

With these developments Pennsylvania assumed undoubted leadership in forestry at the state level. As such the influence of Michaux extended far beyond the borders of the state. In the *Sylva* Michaux provided a large foundation stone to scientific forestry in America; and through the legacy he contributed powerfully to the development of that degree of popular support for forestry without which the first could not come into being.

There is not time even to mention the other uses to which the income from the Michaux Fund was put in the earlier years. In 1899 a change in policy was instituted whereby the income, less 10 per cent for reinvestment, was made available for purchase of books on agriculture and forestry for the Library of the Society. And finally in 1941 the present policy was inaugurated under which the available income is administered by the Society's Committee on Research for appropriate research projects in accordance with the will of Michaux. If a brief report is submitted at this time on one of these projects, it is because it is intimately related to François-André Michaux on the one hand, and, on the other, concerns oaks—the dominant botanical interest of both son and father. At the same time it will be illustrative of work presently made possible by the Michaux Fund.

In 1953, with support by the American Philosophical Society from income of the legacy, there was inaugurated at the Morris Arboretum of the University of Pennsylvania in Philadelphia a long-time research project on oaks which at the same time will constitute a memorial to François-André Michaux. The memorial, under the name Michaux Quercetum, takes the form of a collection of oak species of the world adapted to our climate and soils and so assembled, documented, and controlled as to have optimum scientific value. That a memorial expressed in oaks is appropriate to François-André Michaux, and indeed to his father, is apparent. And as to some of the research opportunities such a collection will provide, even during its formative stages, these will become apparent in the following brief account. A mere planting dedicated to Michaux, however well designed and executed, would fall far short of realizing the possibilities of the opportunity.

The undertaking was organized as a cooperative one between the Arboretum and the Northeastern Forest Experiment Station of the United States Forest Service. This was logical since the two organizations have long been closely associated. The Station's work and research staff in forest genetics and tree breeding have been located at the

Arboretum since 1942, where are available for breeding and genetic studies large collections of native and exotic ligneous plants.

Assembling the material in every case begins with 100–125 acorns from a single tree in native wild stands, accompanied by precise geographical data and an herbarium specimen from the same individual tree from which the corresponding acorns are collected. Habitat data are also requested, partly as a guide in selecting a suitable planting site in the Quercetum. For each collection so made, collaborators are asked to secure a second complete collection in the same locality from another tree of the same species or variety. If in any case the two replicate collections from a given locality show similar significant departures from the type, this would suggest the possible existence of a true-breeding race within the species. In that event, efforts will be made wherever possible to secure sufficient additional collections from the locality statistically to prove or disprove the existence of a race.

From each collection received a few acorns are retained for the herbarium. The rest, certain data having been recorded (including weight and other characteristics), are planted in replicated rows in special seedbeds, of which there are now five, each 100 feet long and 4 feet wide. It is apparent that the seedlings developing from each collection are single-tree progenies. As already indicated, the Arboretum and the Station are interested in the existence of true-breeding races within species. Especially in the wider-ranging species, therefore, serious effort is made to secure collections from various regions in the natural distribution areas of the respective species.

Widely scattered volunteer collaborators—botanists and foresters—do the collecting. In the first year (1953), both to gain experience and to get under way promptly, work was limited to the United States and Canada. Since that time collaboration has been arranged farther afield—in the temperate Orient, in Europe, and in the Near East. At best it will require years to secure acorns of something approaching all species which should be tested. The habit in many species of fruiting only intermittently also causes delays.

In 1953, 1954, and 1955, 410 acorn collections were received, representing 42 species and 10 varieties, and coming from 33 states in the United States, from Canada, and from 7 European and Near East countries. Ninety botanists and foresters made these collections, with every indication that the detailed directions supplied for collecting and forwarding were carefully observed in nearly all cases.

Detailed periodic studies of the seedlings from each collection are begun as soon as the plants emerge in the seedbed. These are continued for three or four years more in the nursery, to which the seedlings are transplanted the second spring. Data recorded include, among others: time of germination; growth characteristics, including rate; leaf and stem morphology; color; hardiness; time of leaf fall; etc. And periodically specimens (in the earlier stages whole seedlings) are deposited in the herbarium. There is thus accumulating unusually complete and herbarium-documented information on the early developmental history of oak species as represented by one or more single-tree progenies, including the ephemeral juvenile stages. At the same time these studies serve to eliminate hybrid material, since non-hybrid germ plasm is desired for the breeding and other research objectives of the collection. Only progenies which prove true to type and show no evidence of hybridity are retained.

Since the seedling studies involve statistical considerations, it is important to reduce to a minimum losses from causes other than climatic unfitness. Seed and seedling losses in the seedbed are prevented by constructing the beds rodent proof; and losses in transplanting from seedbed to nursery have been practically eliminated through a device in the specially designed seedbeds which avoids all root mutilation when lifting the seedlings.

There is not time to report on the technical data currently accumulating. From time to time these will be prepared for publication, some preliminary results having already been published. But it should be pointed out that also during its early stages the project promises to yield a substantial body of valuable information, apart from research envisioned once the definitive collection is well established.

This definitive collection, i.e., the Michaux Quercetum, will contain not more than three trees of any one species, variety, or race. And as noted above, only those will be admitted which during the study of the single-tree progenies in seedbed and nursery prove true to type and not sufficiently variable to indicate hybridity. The objective of the Quercetum is not to assemble a collection of pure lines; even if it were, the impossibility in oaks of producing a rapid succession of inbred generations renders this impractical. But it will be an unusually authentic and documented collection of

species and germ plasm. As such its value for research is apparent, not only for breeding but in many other directions as well. The equally authentic surplus plants will be distributed to institutions desiring them and experienced in maintaining such collections. Besides making this material more widely available for educational and research purposes, this distribution provides insurance that these valuable certified stocks will not be lost should hurricane or other disasters strike in Philadelphia.

That the Quercetum is designed to be scientifically productive over a long period will not detract from the honor it does Michaux, to whom it is specifically dedicated. On the contrary, I think we may be quite sure that, were he here, Michaux would be reassured that the memorial his bequest is helping to create gives promise of long contributing to scientific forestry at the fundamental level.

About eighty-five years have passed since the Michaux Fund came into the hands of the American Philosophical Society. Representing a capital sum of $6,000, the income could not be other than modest. Yet it must be conceded that its influence in the development of forestry in America has been impressive—out of all proportion to the expenditures involved. And, happily, its future potential is undiminished. In fact, the fiscal administration of the bequest by the Society is itself an enviable example in conservation: over the years most of the income has been used, while the capital by the end of 1955 had increased to about $85,000. Granted wise use of the income, it is reasonable to expect that the prophetic foresight of Michaux will continue to be a creative force in the development of forest conservation.

In making his American bequests, Michaux in his will states that he is motivated in part by his gratitude for the hospitality and help he and his father had received in the United States. A century has elapsed since the drawing of this part of his will. It is altogether fitting that we in America in turn express our profound gratitude for the contributions of this illustrious Frenchman to the emergence and development of scientific forestry in America. This expression, however, falls far short of that of Michaux. Unfortunately it lacks the effective implementation with which he so generously endowed his tender of gratitude.

ANDRÉ AND FRANÇOIS-ANDRÉ MICHAUX AND THEIR PREDECESSORS

An Essay on Early Botanical Exchanges between America and France *

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THE part played by the forest in the social and economic life of Europe has often been studied. The author of the following remarks would have never dared to submit them to a conference of specialists if he had not become convinced through a long acquaintance with the literature of travel, that the history of botany, and particularly of botanical discoveries, has not always obtained the recognition it deserves on the part of general historians.

The first travelers who paid attention to the productions of the soil did not claim to be scientists. They were impelled by a naive curiosity as well as by utilitarian considerations. They looked for plants which they could use for food or remedies, for trees which would provide timber for civil and naval constructions. Timber up to a recent date was as essential to the life of Europe as steel has become to us. The first discoverers and the first settlers were not all dazzled by a golden mirage of an enchanted Eldorado. They were aware that from the soil of the newly explored lands they could obtain rich resources to supplement the dwindling supplies of their mother countries.

Their ambition was not limited to the exploitation of the colonies and the lands they visited. They attempted to transplant and to acclimatize exotic productions in their home gardens and forests. At the same time they brought over with them the plants with which they were familiar. Thus, very early and most definitely at the end of the sixteenth century, was established a reciprocity giving an ambivalent character to the botanical exchanges between the New World and Europe. We purposely say Europe, for, although these remarks deal only with France, botanical exchanges were always carried out irrespective of frontiers, political divisions, and conflicts. This was particularly true, as we shall have occasion to remark, concerning the relations between French and English botanists.

Thus, the field open to the historian even when limited to North America and more particularly to the territory which later became the United States is extraordinarily rich. No attempt can be made to cover it in its entirety in this brief report. We must keep in mind that the chief purpose of this meeting is to pay a special tribute to two men who were pioneers and precursors. Nevertheless, they also had precursors, some of whom deserve at least a brief mention.

I. EARLY TRAVELERS AND MISSIONARIES

Let us recall, just for the record, that beginning with Champlain, the first French colonist who settled in Canada brought with them the trees and plants of the French gardens and orchards. The missionaries in general and more particularly the Jesuits were no less active in this field. Their contribution to the knowledge of botany has never been studied in detail. It has been indicated in a German dissertation whose author was killed during the First World War, Walter Hanns, *Die Verdienste der Jesuitenmissionare um die Erforschung Kanadas. Ein Beitrag zur Entdeckungsgeschichte von 1611–1759,* Iena, 1916. The author has devoted a chapter to the plants described or mentioned in the Jesuit relations (p. 90–94) and including in his survey such outstanding titles as Jacques Philippe Cornut, *Canadensium plantarum historia* (Paris, 1635) and the more general account of Nicolas Denys, *Description géographique et historique des côtes de l'Amérique Septentrionale avec l'histoire naturelle de ce Pays* (Paris, 1642). It cannot be doubted that the work of the missionaries was not limited to the "propagation of the faith." They were practical men, intent upon developing the resources of the countries in which they worked *ad majorem Dei gloriam.* Although little known, their very effective contribution to the botanical knowledge of the

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countries they explored should not be minimized.

All the botanical exchanges carried out between the old and the new world were not carried out through direct channels. Close connections were maintained during the seventeenth century between French and English scientists. It would be more exact to say that the universalist spirit of the Renaissance continued to manifest itself during the whole century. In a recent book replete with original views and little known documents, Michael Kraus has endeavored to trace *The Eighteenth Century Origins of the Atlantic Civilization* (Cornell University Press, 1949). He has devoted a whole chapter to scientific relations and particularly to the relations between Dutch, French, and British botanists (p. 163–175). In fact, this so-called Atlantic civilization began, as far as the "naturalists" are concerned with the first decades following the discovery of the New World. Its development through the eighteenth century was the regular continuation of a tradition already well established rather than a new departure.

The foundation of the Royal Society of London, followed in France with the creation of the Académie des Sciences, brought even closer together the French and the British scientists. My colleague, Gabriel Bonno, in a recent monograph, *Les Relations intellectuelles de Locke avec la France d'après des documents inédits* (Berkeley, 1955), has revealed the botanical interests of the English philosopher. During his visit to France he had become very intimate with Dodart, one of Louis the Fourteenth's physicians, and had started with him a regular exchange of plants and seeds. He kept up a regular correspondence with Jutel, a rich amateur rather than a true scientist. From this correspondence I shall quote only one passage which seems particularly revelant. In a letter written November 23, 1679, Jutel wrote Locke: "Vous me ferés plaisir de me mander s'il y a un arbre en Angleterre qu'on nomme l'arbre Tulippe et si on voudroit le troquer contre d'autres plantes curieuses que M. Marchant donnera volontiers." The Marchant mentioned here was Jean Marchant, son of Nicolas Marchant, curator of the Jardin du Roi, who had died during the preceding year. An earlier work of Gabriel Bonno contains on our subject even more interesting data: *"La Culture et la civilisation britannique devant l'opinion française de la paix d'Utrecht aux Lettres philosophiques,* 1713–1734" (*Transactions* of the American Philosophical Society **38** (1), June, 1948). One will learn there that Robert Morison, the author of the *Historia Plantarum Universalis* (Oxford, 1680–1699), had been director of the gardens of Gaston d'Orléans at Blois, before becoming professor in Oxford. One will also see how Hans Sloane remained all his life a most active liaison agent between the two countries. He had studied in Paris with Tournefort and Pierre Chirac, and with Magnol in Montpellier. Later he kept up a regular correspondence with François Geoffroy, Antoine de Jussieu and abbé Bignon. He exchanged with them seeds and plants and his studies were regularly mentioned in the *Journal des Savants* and the *Mercure*. More information on the subject may be obtained from a recent article by Jean Jacquot: "Hans Sloane and the French Men of Science," *Notes and Records of the Royal Society of London* **10** (2), April, 1953. Unfortunately the author of the article seems to have been unaware of the investigations carried out by Gabriel Bonno, but he has made a summary inventory of that rich correspondence which should be integrally published. Obviously, the French botanists of the time had only secondhand information on the flora of the British colonies but there is also ample evidence that at the end of the seventeenth century they had succeeded in obtaining rich collections of specimens from that exotic flora through their British colleagues.

The attention of the French Huguenots was naturally drawn to the settlements in which the British colonists had succeeded in finding both religious peace and living conditions far better and more promising than they could enjoy in their mother country. It is not surprising that the first relations written in French and dealing with the British colonies, were published by Huguenots for whom America already loomed up as the land of refuge by excellence. When the Calvinist minister Rochefort published the second edition of his *Histoire naturelle et morale des Antilles* (Rotterdam, 1681), he appended a 43-page supplement entitled "Récit de l'Estat present des celebres colonies de la Virginie, de Marie-land, de la Caroline, du Nouveau Duché d'York, de Penn-sylvania, et de la Nouvelle Angleterre." Rochefort had lived for twelve years, between 1636 and 1648, in the West Indies. It does not appear that he ever visited the continent, but he had collected reliable information and he endeavored to provide his coreligionists, threatened in their lives and property, with exact data on the lands where they could find for themselves and their families, *panem and libertatem*, bread and liberty. He was no botanist and he limited himself to a bare enumeration of

the trees that could be used in the building of ships and houses. He also noted the fruit trees and the vegetables, mostly imported from England, that were already cultivated in the colonies; but above all the marvels of the New World, he places "une espèce de bled" that the colonists were beginning to grow and "qu'on appelle Mays."

Almost at the same date another traveler published his *Voyages d'un Français exilé pour la religion avec une description de la Virginie et Marilan dans l'Amérique* (La Haye, 1687), of which there is a modern edition, Paris and Baltimore, 1932. Durand de Dauphiné, such is the name by which he is known, devoted a whole chapter to the American forest, noting that "La terre de ce pays est entièrement couverte d'arbres, à la réserve des fons que l'on cultive où on les a coupés." The pine trees there are prodigiously tall and straight, the live oaks are found in abundance, chestnut trees grow everywhere, there are three species of walnut trees, cedars and fruit trees are found on all the plantations. It would thus appear that Durand de Dauphiné might rightly be considered as the earliest and most authentic predecessor of the Michaux. It goes without saying that he had no scientific pretension. His interest in plants and trees is limited to those that can be utilized by eventual refugees. One may surmise, however, from the chapter entitled "Beauté et fertilité de l'Amérique," that the charm of the primeval forest did not leave him completely indifferent.

We must return now to the achievements of the missionaries, explorers, and colonists who, from the end of the seventeenth century to the treaty of 1763, established French settlements on the southern shores of the Great Lakes and in the Mississippi Valley. Pending a more complete investigation one might resort to the monumental *Histoire et Description générale de la Nouvelle France, avec le Journal Historique d'un voyage fait par ordre du Roi dans l'Amérique Septentrionale* (Paris, 6 vols., 1743), with a dedicatory epistle dated October 15, 1743. The fourth volume contains the "Description des plantes principales de l'Histoire de l'Amérique Septentrionale." As most of his contemporaries and predecessors, Charlevoix deals chiefly with useful, harmful or medicinal plants, indicating in each case the essential characteristics permitting to identify them. We shall simply note here that he did not list less than eight varieties of oaks. For these chapters as well as for the purely historical part of his great work, he consulted the relations of his predecessors. The "Voyage" printed in the sixth volume is more original. His itinerary from the junction of the Missouri with the Mississippi deserves special attention. On his way Charlevoix noted different species of forest trees, always with the preoccupation of marking their eventual utilization, but occasionally with a sense of picturesque values seldom encountered in the relations of that period. At Kaskakia, he remarked that the white mulberry trees provide good timber, since the colonists fell them in order to build their houses. Farther down on the river, he came to the cypress forests "dont les troncs dépassent tout ce que nous avons en France de plus grands arbres." He did not overlook "la racine de ces grands cotonniers qu'on ne cesse de trouver dans toute la route que j'ai faite depuis de Lac Ontario, un remède assuré contre toutes sortes d'écorchures." He grew enthusiastic about the medicinal virtues of

> la Cassine, autrement nommé Apalachine (*Ilex vomitoria*) dont la feuille infusée comme celle du Thé, parce que c'est un bon dissolvant est un excellent sudorifique. . . . Elle commençait à faire quelque fortune à Paris lorsque j'en suis parti. Je scai que bien des personnes qui font usage de l'Apalachine s'en louent beaucoup (VI, 221).

Besides these practical considerations are found impressions of an entirely different order. In Canada, Charlevoix had been impressed by the grandeur of the primeval forest:

> Nous sommes au milieu des plus grandes forêts du Monde, selon toutes les apparences; elles sont aussi anciennes que le monde même, et n'ont point été plantées de mains d'Hommes; à la vue rien n'est plus magnifique, les Arbres se perdent dans les nues, et il y a une variété d'espèces différentes si prodigieuse, que parmi ceux-mêmes qui se sont le plus appliqués à les connoitre, il n'est peut-être personne qui n'en ignore plus de la moitié (V, 234).

The American silva filled him even with greater admiration:

> Pour ce qui est des Forêts qui couvrent presque tout ce grand Pays, il n'en est peut-être pas dans la Nature, qui leur soient comparables, car on ne sçauroit dire quelle sorte d'Arbres on n'y voit pas.

Among all the trees he singled out "l'arbre tulipe" of which Jutel had requested seedlings from Locke. The tree was probably already acclimatized since Charlevoix added:

> On commence à connoitre en Europe cette espèce de Laurier toujours verd que nous avons appelé Tulipier à cause de la figure de sa fleur. Il s'élève

plus haut que nos Maroniers d'Inde, et sa feuille est encore plus belle. Le Copalme est encore plus grand et plus beau (VI, 162).

One may wonder whether this description would not fit better the *Magnolia grandiflora*, later so magnificently described by Chateaubriand in *Atala*, rather than the tree now known in the United States as the tulip tree.

Charlevoix went down the Mississippi between October, 1721, and February, 1722, a very few years, one might almost say a few months, after the foundation of New Orleans. Four years earlier, exactly in 1718, an agent of the Compagnie des Indes had been sent to Louisiana by the General Director M. de la Chaise, with a special mission, namely to collect and send to France, all the seeds, seedlings, and plants which in his opinion could be grown profitably in the Old World. Le Page du Pratz had no particular knowledge of botany, but he did his best to fulfill his mission. The three volumes he published in 1758 under the title of *Histoire de la Louisiane*, are replete with observations and details from which we may be permitted to select a few.

Of particular value is the notation that he had

transplanté en terre dans des paniers de canne 300 simples avec leurs numéros et un Mémoire qui détailloit leurs qualités et enseignoit la manière de les employer. J'appris qu'on les avoit mis dans un jardin botanique fait exprès par ordre de la Compagnie (I, 212).

I have not been able to identify this "jardin botanique," but one may wonder whether this shipment was an isolated case, and whether the archives of the Compagnie des Indes would not provide details on similar experiments, and perhaps lists of plants of the greatest interest?

As Charlevoix had done, Le Page du Pratz roamed through the American forests, but unlike Charlevoix he did not indulge in poetical effusions. He noted four varieties of walnuts and four kinds of oaks which dominate in the "futayes," but it is only to indicate at once that, on the stump of the walnuts grew a particular kind of fungus carefully collected by the natives and which seemed to possess special medicinal virtues (I, 285). He noticed the long trains of the moss commonly called "barbe espagnole" hanging from the trees, so poetically described by Chateaubriand, only to compare them quite prosaically to disheveled old wigs "vieilles perruques." He found some use for it, however, and managed to stuff his mattress with it. He was interested in agriculture and indicated how to cultivate "Mahiz," rice, "la fève des Apalaches," watermelons, indigo, and cotton (III, ch. 20, 21, 22). He was not entirely devoid of esthetic appreciation. He praised the beauty of the "laurier à Tulippes" which could be used to make "des allées dignes d'un jardin royal." But he admitted that he did not pay much attention to the "fleurs qui enchantent la vue." He made an exception for the one which is called "Gueule de lion":

Ses couleurs nuancées, sa durée de plus de trois mois me la font préférer à toutes les autres. Elle fait à elle seule un bouquet très agréable. Je la crois digne du rang de beaucoup de fleurs très belles, et que l'on cultive dans les jardins de nos Rois (II, 65).

Le Page du Pratz adds that he sent plants and seeds of "gueule de lion" to the Compagnie des Indes, but I must leave it to the botanists to find out whether it was acclimatized and propagated in France.

Our amateur plant collector arrived in Louisiana on August 25, 1717; he went back to France in the spring of 1734 and landed at La Rochelle on June 17 of the same year, after a stay of seventeen years in the colony. Little is known about the rest of his life, except for passing allusions in his *Histoire de la Louisiane*. One detail, however, may provide an interesting lead to local historians. We learn from him that he settled in Fontenay-le-Comte and there strove to grow in a sort of private experimental garden the seeds he had brought with him from Louisiana. He even gave some of them to the head gardener of the Intendant du Poitou. We do not know to what extent he succeeded in propagating in the region these exotic plants. Perhaps some indications could be found in the publications of the agricultural societies of Poitou and Saintonge. We can only hope that some local historian interested in botany will be tempted to undertake this research. While he was collecting plants for the Compagnie des Indes, Le Page du Pratz undertook to naturalize French plants in the Mississippi Valley. This brings up a problem which I can only indicate, leaving to more qualified investigators the responsibility of studying it in detail.

One may wonder indeed to what extent the French colonists and explorers have contributed to the enrichment of the American flora, in the region which extends from the Great Lakes to New Orleans? Local traditions attribute to them the introduction of many kinds of fruit trees some

of them still cultivated. This tendency is manifest in a recent book by U. P. Hedrick, entitled *A History of Horticulture in America to 1860* (New York University Press, 1950), in which will be found a bibliography of the subject. I shall refrain from venturing on controversial ground where a mere historian would encounter many pitfalls. I shall simply mention the fact that a very different hypothesis has been proposed in a long study published almost a century ago by Henry Ward Beecher, *Plain and Pleasant Talk about Fruits, Flowers and Farming* (New York, 1859). The author, who lived for twenty years in Indiana and traveled much around, declared positively that most of the trees and plants cultivated in the region came from the Eastern States and to the Eastern States from England. He quoted specifically from a letter on the "Progress of Agriculture in Indiana," published in *Hovey's Magazine of Horticulture*, February, 1845. An investigation of that kind is made exceedingly difficult by the fact that in many cases the French names of certain varieties have been anglicized to the point of becoming unrecognizable. To give only one instance, the *Belle-fleur*, apple tree, which seems to have come from Canada through Detroit, has become *Bell Flower* through a strange metamorphosis under which the French origin has disappeared. Let us also remember that the French settlements of the Mississippi, already in a very precarious situation before the treaty of 1763, were submerged under the great Western migration following the Louisiana purchase.

II. EARLY ORGANIZATION OF THE EXCHANGES

All in all, one must look elsewhere to study the history of the botanical exchanges between France and the New World. Eighteenth-century science was international as well as encyclopedic. The "Republic of letters" which included the sciences remained for a long time above political vicissitudes, wars and national rivalry. The governments themselves practiced this high-minded and disinterested policy, with the happy consequence that the long lasting and repeated conflicts between France and Great Britain, failed to modify to any degree the personal relations between the scientists of the two countries. As a point of fact one of the first results of the bitter fight which was waged for the possession of Canada and the immense region extending South of the Great Lakes was to establish direct connections between the French and the American botanists. In these exchanges two Philadelphians, John and William Bartram, played an outstanding part, compatible in every respect to the part played later by André and François-André Michaux. Bartram, father and son, whose activities extend over the greater part of the eighteenth century, were at the same time explorers, botanists, and nursery men, drawing a legitimate profit from the plants they had acclimatized in their Philadelphia gardens. Their complete biography remains to be written and the many manuscripts which are preserved in Philadelphia are still to be published. Some idea of their scientific activities may be gathered from the *Memorials of John Bartram and Humphrey Marshall*, published more than a century ago by William Darlington, (Philadelphia, 1849). The *Memorials* contain many curious details. The following one is very significant. When in 1738 a young Maryland physician recently settled in Philadelphia, Dr. Thomas Bond, went to France to study with Jussieu, the French botanist helped him to identify several American plants marked "incognita" by Bartram. The Jardin du Roi at that time was so rich in specimens of the American flora that Dr. Bond could believe himself miraculously transported back to his native surroundings, and he could write to John Bartram:

> My friend Jussieu tells me that I shall believe myself at home, by being amongst so many of my native plants, as brought from America by himself, in quest whereof he was sent by the King (Paris, Feb. 20, 1738, Darlington, p. 317).

In Darlington, again, is printed a precious letter of Peter Kalm to John Bartram, dated Quebec, August 6, 1749. The Swedish botanist took pleasure in informing his American friend that the King of France had given orders that the foreign scientist be received, entertained and transported free of charge during his whole trip: "It is not permitted to me to pay anything, but the King of France he pays that all" (Darlington, p. 317). The generous treatment granted Kalm was no more than a fair return for similar favors received by French scientists when they were traveling in Sweden.

On April 20, 1755, Bartram wrote his friend Philip Miller, chief gardener of the Gardens of Chelsea and author of the famous *Gardener's Dictionary* (London, 1731), that he had received from Dalibard a complete catalogue of the plants growing around Paris and that, in exchange, Dalibard had asked him to send him "all the

species of Toxicodendron," a request which will seem very imprudent to those who know through personal experience the noxious plant commonly known as poison ivy and in Canada as "l'herbe aux puces." Some of these botanical additions to the native flora were not always welcome. In another letter to Philip Miller, Bartram drew up "A brief account of those plants that are most troublesome in our pastures and fields in Pennsylvania; most of which were brought from Europe." In this list one will recognize the plants of the French roadside and meadows, dandelion, saponaria, and camomille (Darlington, p. 383). It would be easy to multiply these quotations. We made them here simply to illustrate the fact that as early as the first third of the eighteenth century the French botanists, while continuing their exchanges with England, were already in direct communication with John Bartram and through him with the chief American botanists and nursery men.

The archives of the Museum probably contain impressive lists of the American plants already in the Jardin du Roi, but even more significant are the catalogues of the commercial nursery men. Only a few were examined among which the *Catalogue raisonné des plantes et arbres et arbustes dont on trouve les graines, des bulbes et des plants chez le sieur Andrieux* (Paris, 1771). The following species with their American origin clearly indicated are listed: *Cornus florida, Tilia americana, Robinia pseudoacacia*, peuplier du Canada, peuplier de Caroline, platane de Virginie, Viorne du Canada, bignonia de Virginie, grenadille (passion flower), sumac vinaigrier, *Juniperus virginiana*, not to mention the nefarious *Rhus toxicodendron* which Dalibard had probably successfully acclimatized.

American plants were also sought after outside of the Parisian region, as appears from the *Catalogue des arbres fruitiers, arbres d'ornement, arbrisseaux, arbustes, plantes . . . Qui se trouvent dans les pépinières du Sr. Bruneau, fils aîné, agriculteur à la Croix Saint-Marceau, à Orléans*, Orléans, 1784. The good nurseryman offered to his customers fifteen species of American trees, twelve shrubs and five evergreens among which were the "sapiente ou Hemlock-spruce" and "le cèdre rouge de Virginie."

If time and space permitted, this would be the place to analyze a manuscript entrusted to me by Léon Rey a few months before his death. It bears the altogether too modest title of "Deux botanistes français aux Etats-Unis. Les missions de Michaux père et fils (1785–1808)." The introduction is in fact a separate monograph on the development of the royal nurseries during the eighteenth century and presents the result of long and patient researches carried out for more than ten years in the Archives Nationales, the Archives du Muséum, the diplomatic correspondence of the Ministère des Affaires Etrangères, and the Archives de Seine-et-Oise. Léon Rey, however, had realized that an investigation of that character had to be supplemented by a careful study of many documents preserved in the American archives, and he had hoped that the publication of his work in America would lead to similar inquiries on the part of American specialists. His last wish will be fulfilled if as we hope this magnificent piece of research is published by the American Philosophical Society.

Here I shall simply indicate that in the first part of his study Mr. Rey has made a detailed survey of the "jardins français où furent cultivées des plantes américaines" particularly the royal nurseries of Roule, Versailles, and Rambouillet. He has also traced the considerable part played by abbé Rolin and M. d'Angiviller in the propagation of exotic and American plants. From the documents he has reproduced one must at least conclude that the acclimatization of foreign plants was, under the *ancien régime* and more particularly during the second half of the eighteenth century, an integral part of the King's policy and that in many cases it was carried out through the personal intervention of the monarch. One must also remark that these importations were very seldom unilateral. Thus were established almost official relations between the French botanists and the scientific societies of England, for, as we have already indicated, most of the American plants reached France through British intermediaries. Interesting as were the relations of Bartram, Humphrey, and some other American botanists they were necessarily exceptional. The proclamation of the independence of the United States brought about new conditions.

All of a sudden, the English botanists found themselves cut off from their American correspondents, with the consequence that the French botanists were also deprived of indirect but convenient means of procuring American plants. It was of prime scientific interest to establish direct communications between the French scientists and those who were still called the "Anglo-Américains" or the "Insurgents." This preoccupation appears in a very significant manner in a letter written by

Benjamin Franklin to Bartram, a few months after his arrival in Paris, on May 27, 1777. It was published by Darlington, p. 406, but as it seems to have escaped the attention of the editors of Franklin's writings we give it here in full:

My old dear Friend:

The communication between Britain and North America being cut off, the French botanists cannot, in that channel, be supplied as formerly with American seeds, &c. If you, or one of your sons, incline to continue that business, you may, I believe, send the same number of boxes here, that you used to send to England; because England will then send here, for what it wants in that way. Inclosed is a list of the sorts wished for here. If you consign them to me, I will take care of the sale, and return, for you. There will be no difficulty in the importation, as the matter is countenanced by the ministry, from whom I received the list.

My love to Mrs. Bartram, and your children. I am ever, my dear friend,

Yours most affectionately

B. FRANKLIN.

In that ingenious way, the American envoy, with the full assent of a government to whom he had not been yet accredited, managed, in the interest of science, to initiate and foster activities which, in our days, would probably be deemed "commerce with the enemy." Truly the eighteenth century was the Golden Age of the "Republic of letters."

These botanical exchanges were considered so important by the royal government that the first French envoy to the American Congress, Conrad Gérard, was instructed to establish connections with the American botanists and horticulturists. On April 7, 1779, Bartram wrote to his friends, Thomas Bond and Humphrey Marshall, informing them that he had received from the French plenipotentiary a list of plants to be shipped to France. From a later communication we learn that the order was transmitted by Barbé-Marbois the young secretary to the legation and on March 16, 1781, Bond could inform Marshall not only that the shipment had been received in France, but that the King had personally examined all the items of the collection and had been extremely pleased. Bond ended with a patriotic wish: "This is a respectful and may be a very useful corresponding. Let us support it with the patriotic spirit it deserves" (Darlington, p. 537, 539, 556). One likes to assume that, in that circumstance, Dr. Bond remembered the conversations he had with Bernard de Jussieu in the Jardin du Roi, forty-four years earlier.

We are still imperfectly informed on Franklin's participation in these botanical exchanges. A few documents on the subject will be found in the essay I published under the title of *Les Amitiés américaines de Madame d'Houdetot* (Paris, 1924). From manuscripts recently acquired by the American Philosophical Society, it also appears that Franklin was often consulted by his French friends and particularly by Malesherbes and Fougeroux de Bondaroy; but, as we shall see later, it was reserved to his successor in Paris, Thomas Jefferson, to become the most active promoter we have encountered of the botanical exchanges between France and America.

Even before the signature of the peace treaty, the royal government endeavored to place these exchanges on an official footing. Such was, at least in part, the object of the questionnaire sent by Barbé-Marbois to the governors of the thirteen states, as early as 1781. It is well known that Jefferson wrote his famous *Notes on Virginia* in answer to the questionnaire. In answer to the seventh query he gave a catalogue of the native plants, restricting himself to "those which would principally attract notices." Botany also occupies a very important place in the official and semi-official correspondence of St. John de Crèvecœur who was in charge of the French consulate in New York from 1783 to 1790. Léon Rey has transcribed the letters preserved in the Archives de la Marine, in the Archives Nationales and in the Archives Municipales of Mantes-la-Jolie. The chief correspondents of Crèvecœur, were Maréchal de Castries, baron de Breteuil, and the duc de la Rochefoucauld. Crèvecœur was particularly interested in the commercial utilization of forest trees, but he also undertook to impress upon the Americans the ornamental values of the native species. He offered to plant trees on Broadway at his own expense; he maintained regular correspondence with the scientific societies and urged his friends to establish new ones. He is remembered as the author of the *Lettres d'un Cultivateur américain*, published in Paris in 1782; his correspondence will show that he was also extraordinarily active as a scientific and commercial agent.

The end of the reign of Louis XVI was marked by two scientific expeditions of major importance. The first was Lapérouse's voyage prepared with the utmost care, as appears in the instructions written by Thouin "pour diriger le jardinier dans son voyage autour du monde." America occupies a comparatively small place in the memoirs published after Lapérouse's death (2 vols. an VIII),

This part has been reprinted separately under the title of *Le Voyage de Lapérouse sur les côtes de l'Alaska et de la Californie* (Paris and Baltimore, 1937). Collignon, who was the "gardener," gave a very brief description of the flora of California. But the stay of Lapérouse at Monterey constitutes an important episode in the history of botanical exchanges. In accordance with the King's definite instructions, Lapérouse had taken aboard seeds and plants packed according to the method indicated previously by Duhamel du Monceau. Thus it was possible for Lapérouse to present the Franciscans of the Carmel mission with seeds and seedlings of fruit trees to plant in their orchards in return for the provisions obtained from the monks for the French expedition.

II. ANDRÉ AND FRANÇOIS-ANDRÉ MICHAUX

The royal government and more particularly comte d'Angeviller who as head of the "maison du roi" had under his jurisdiction the parks and gardens, soon realized that despite their zeal neither the French Consul in New York nor the minister plenipotentiary could devote sufficient time to the important problem of the botanical exchanges. Perhaps I may be permitted here to refer to an article published a few years ago in which I attempted to show that the problem of the forest had become vital and that without an adequate supply of timber France could never hope to cope with England on the high seas. Obviously France had to obtain at once a large additional quantity of wood to maintain and develop her fleet ("The American Philosophical Society and the Early History of Forestry in America," *Proceedings* **89** (2), July 18, 1945). This aspect of the question has been recently studied more in detail by Mr. Paul Walden Bamford, *Forests and French Sea Power* (University of Toronto Press, 1956). But it was also a matter of long range policy; in final analysis, in order to be secure France had to develop her domestic resources and obtain from her own soil the precious material which would permit her to keep afloat her merchant fleet and her navy. Despite the representations of the French scientists among whom were Buffon, Duhamel du Monceau, the members of the Jardin du Roi and the Académie des Sciences, the French forests had been depleted. Oak trees without which no strong vessel could be built were of slow growth. This alarming condition could perhaps be remedied in part if trees of more rapid growth were introduced and planted in the royal forests. The travelers and the officers who had participated in the War of Independence had expressed in lyrical terms their admiration for the American forest. But only a well-trained botanist and "gardener" could determine the possibilities of transplanting and acclimatizing foreign species. At this juncture comte d'Angeviller discovered the ideal man for the job. André Michaux was not perhaps an outstanding botanist, but he was a well-trained and practical "jardinier." He had just returned from a voyage in Persia during which he had evidenced a rare aptitude for learning foreign languages and an incredible physical resistance. He was then slightly under thirty-nine and was looking forward to a new voyage in Asia Minor. Without any hesitation he accepted a mission of an entirely different order. He was not expected to discover a new flora or to explore unknown regions, but more prosaically and practically to establish in a well-chosen spot nurseries, in which he could study *in situ* the conditions under which American trees could grow and be acclimatized.

We cannot here sketch even briefly the American career of André Michaux; we shall limit ourselves to summary indications on the state of the question.

Taking along with him his young son, François-André, a "jardinier" by the name of Saulnier, and a young servant Jacques Renaud, André Michaux sailed from Lorient on September 27, 1785. He landed in New York on November 14; he was to stay in America for almost ten years. At once he displayed an extraordinary activity. He rapidly explored the surrounding country, decided that New Jersey presented acceptable conditions for the establishment of a "garden," collected seeds and plants and as early as December sent a first shipment to Paris. On March 2 of the following year, the Assembly of the State of New Jersey authorized Michaux, acting for the King of France, to purchase the piece of land he had selected, although, according to the laws of the State, no foreigner was permitted to own any land. Michaux spent the first months of 1786 laying out his garden; he built on it a very modest house for the gardener, and a greenhouse for the plants he expected to obtain from the South. At the same time he presented himself to the French Minister in Philadelphia, visited the gardens of Bartram and was received by Franklin and Washington to whom he was bringing letters of introduction.

On September 6, 1786, he sailed for Charleston and judged it the ideal place to lay out a botanical

garden. He acquainted the French plenipotentiary in Philadelphia with his project and, without waiting for any official authorization, bought outright a plot of 111 acres, ten miles from Charleston. The deed which I found in Charleston is dated November 3, 1786.

From that date on, he made Charleston his headquarters. We shall not follow him in his many trips through all the accessible regions of the United States. His journal gives ample proof of a prodigious activity and physical endurance. When his horse fails him he goes on on foot, covering often more than thirty miles in a day without abandoning a piece of his equipment and managing to note on his way all the plants which meet his sight. The subsidies which he had been promised came irregularly and with the fall of the royal government were completely stopped. But he never lost heart. From 1785 to 1793, according to his calculations, he shipped to France 60,000 plants and 90 boxes of seeds which were distributed through the royal gardens by comte d'Angiviller and abbé Nolin. Just at a time when the situation seemed desperate and he had exhausted all his resources, he formed the ambitious project to explore the unknown territories of the West and to seek the famous and elusive northwest passage. To start with, he intended to seek the source of the Missouri River and to follow the course of the streams which flowed in the direction of the Pacific Ocean. The story of this dream and its subsequent failure has never been fully written. Let us simply recall that he interested Jefferson in his undertaking, that Jefferson asked his colleagues of the American Philosophical Society to subscribe individually to the expenses of the expedition. To determine exactly the role played by Jefferson in this extraordinary adventure, it would be necessary to analyze many documents still unpublished and scattered between different collections. However, it is not out of place to recall that Michaux's project was very similar to another project sponsored by Jefferson some ten years later, the well-known Lewis and Clark expedition. It is impossible to tell whether Michaux would have succeeded, practically unsupported and traveling by himself, in crossing the continent. He firmly believed that the dangers could not be greater than those he had encountered in his travels in Asia Minor and that the Red Indians could not be more hostile than the tribes and brigands he had met in the Orient. The arrival in Philadelphia in May, 1793, of a new French plenipotentiary sent by the Revolutionary government brought about a change in the plans of Michaux and gave a political character to his trip to Kentucky. In fact he agreed with some reluctance to participate in a real conspiracy the ultimate aim of which was to free the Mississippi region and Louisiana from Spanish occupation. The whole project fell through even before it really started and Michaux who had left Philadelphia on July 15, 1793, was back on December 16 of the same year. The diary he kept during these five months testifies that botany rather than politics was at all times uppermost in his preoccupations during the whole trip.

He spent two and a half years longer in America. He had to give up sending to France botanical material; he had no longer any official mission and naturally received no support from the French government. He managed however not only to live, but to keep his botanical gardens and nurseries in working condition. He even undertook all by himself a new trip westward, reached the Mississippi, visited the miserable French settlements remaining along the river. Meanwhile, the French authorities had reached the conclusion that his nurseries were badly located and presented no practical interest. When he finally sailed for France on August 13, 1795, he left the Charleston garden in a very precarious condition; the Bergen garden subsisted only through the ingenuity and tireless efforts of Saulnier.

He was shipwrecked on the coast of Holland and the major part of his collections and of his papers were lost. Back in Paris he experienced another cruel disappointment: the plantations of American trees in the royal forests and parks, and particularly in Rambouillet, had been almost utterly neglected or destroyed during the Revolution. But even then Michaux was not discouraged; he was tireless in his efforts to convince the government of the value and interest for the nation of continuing the work he had started in his American nurseries and he even planned to return, join Saulnier, and start all over again. He finally agreed in 1800 to participate in Baudin's expedition to New Holland. He did not go farther than Madagascar where he died in 1802 of a malignant fever.

His biography remains to be written even for the American part of his life. In France he is known to the specialists through the memoir published by Deleuze in the *Annales du Muséum* in 1804. The study of Michaux made by Léon Rey, still in manuscript form but soon to be published, will provide precious source material obtained

from the French archives. Léon Rey, however, was not able to write the complete history of the Bergen nursery, and to retrace the efforts of Saulnier to continue the work of his chief. Pending a more elaborate survey, one may refer to the sketch of H. H. Torrey, "Michaux's New Jersey Botanical Garden," *Torrey Botanical Club Bulletin*, **11**, 1884, which gives an account of the state of the garden at that date. Further research undertaken recently by Professor William J. Robbins, Director of the New York Botanical Garden, will bring out much additional material on the subject.

More information is obtainable about the Charleston Garden—first from Michaux himself, in the part of his journal which has been preserved. It was presented by his son to the American Philosophical Society in 1824, edited by Sargent and published by the Society (*Proceedings* **26**, 1888). It is a priceless document. Beside the botanical observations which were the principal object of the journal, the historian will find in it vivid sketches of the life of the pioneers and a true although drab picture of what remained of the French settlements in the Illinois region along the Missouri and the High-Mississippi. One must note, however, that Sargent seems to have been unaware of the existence of several pages of notes and of several fragments which are part of the collection. In an article entitled "The Garden of André Michaux" (*Journal of the Elihu Mitchell Scientific Society*, July, 1911), Professor W. C. Coker, of the University of North Carolina, has summed up several articles by other authors and has described the condition of the garden at that date. A more recent article by Judge Henry A. M. Smith in *The Historical and Geneological Magazine* **29** (1), January, 1928, gives a history of the plot. I am now able to supplement it with indications obtained from the local archives by Miss Anna Rutledge. Ten years ago, the outline of the seed beds, a few trees, and the foundations of the greenhouse still subsisted. Steps were to be taken to restore the garden and to preserve the trees; the project had to be abandoned when the lot was taken over by an aviation camp during the last war.

Beside material in the Archives Nationales and the Archives du Ministère des Affaires Étrangères which was analyzed by Léon Rey, one should refer, chiefly for the Kentucky imbroglio, to the Genet papers and the Jefferson papers in the Institute of History and Art of Albany, the Library of Congress, and the American Philosophical Society. Two printed works bear the name of Michaux: *Histoire des chênes d'Amérique*, printed in 1801 after his last departure from Paris, and the *Flora Boreali-Americana* published after his death, in 1804. There is no known portrait of André Michaux, although the administration of the Muséum had voted in 1804 that a bust of the "voyageur" would be placed in the garden.

Whether André Michaux had been somewhat hasty in agreeing to participate in a new expedition or whether he had already been assured that his son might be given an opportunity to take up his American work is a matter for speculation. In any case, the record indicates that the French government had not decided to give up outright the French gardens in America. Early in 1801, François-André was commissioned to study on the spot the condition of the French botanical establishments in the United States and to study the trees which eventually might be planted to enrich the French forests. No more qualified voyageur could have been chosen. Born in 1770, François-André was fifteen when his father took him along with him as his assistant in 1785. He accompanied on several of his trips the tireless traveler and was put by him through a relentless training. From Saulnier he had learned the practical side of horticulture, and back in Paris, he had studied with the botanists at the Muséum. He sailed for Charleston in August, 1801, and landed after a forty-three days crossing.

The Charleston garden had only a nominal existence; it would have been entirely destroyed if a neighbor of André Michaux had not volunteered his good offices and had not taken care of it after a fashion. The French government was not interested in the semi-tropical trees which grew there and obviously could not be of practical value for the development of the French forest. The situation of the Bergen garden was entirely different. It had been kept in working order by Saulnier who had enlisted the active interest of Du Pont de Nemours recently arrived in the United States. Here again Léon Rey's study will make available several striking documents which had hitherto escaped the biographers of Michaux. Following his father's example, François-André acquainted the American Philosophical Society with the object of his mission and was welcomed by the Philadelphia botanists with whom he had previously worked. In New York, he visited Dr. Hosack and Dr. Mitchill and "took a botanical excursion into New Jersey." He then undertook a travel "To the West of the Alle-

FIG. 1. Portrait of François-André Michaux in the Hôtel de Ville of Pontoise. Artist unknown.

ghany Mountains, in the States of Ohio, Kentucky and Tennessea, and back to Charleston," the account of which was published in Paris in 1804 and the following year in London. He was back in Charleston in October, 1802, sent to France many boxes of seeds and plants, and sailed on his return journey in the spring of 1803. He was also bringing back with him a rich harvest of first observations published in part in 1806, in a *Mémoire sur la naturalisation des arbres, arbustes et plantes de l'Amérique Septentrionale*. The title is significant for it indicates that François-André had taken up in earnest the work of his father. Even more significant was the recognition he received the same year from the "Administration des Eaux et Forêts." As "agent temporaire de l'Administration dans l'Amérique Septentrionale," he was instructed "to collect and send to France the forest trees susceptible of being naturalized on waste land (terres incultes) where the native species refuse to grow." Thus François-André was officially associated with the great work of reclamation of the region between Bordeaux and Bayonne undertaken by Brémontier.

He sailed from Bordeaux on February 5, 1806, on an American ship which was stopped and boarded by an English frigate, the *Leander*. France was at war with Great Britain and while the ship was permitted to go on her journey, the young botanist was detained and taken to the Bermudas. He suffered no hardship and had no reason to complain since he was courteously authorized to land and engage in botanical excursions in the islands. He was finally released and landed in Philadelphia at the beginning of July.

He went at once to New York and settled for a few weeks in the little house still occupied by Saulnier, but soon he resumed his botantical explorations, traveling from Maine to Georgia, crossing the mountains and following the Ohio River, reaching the source of the Savannah River and finally cutting across a scarcely settled territory to reach the Great Lakes. Meantime, he conscientiously acquitted himself of his mission; in 1807, twenty-five boxes had reached France and more were on the way. He came back to France in 1808 and devoted himself to the preparation of his monumental work, *Histoire des arbres forestiers de l'Amérique Septentrionale*. The first volume was published in Paris in 1810; two other volumes came out of the press in 1813. He was fully aware of the practical value that his work presented for the exploitation of the American forest and he undertook at once to have it published in the United States. To that effect he carried out a long correspondence with John Vaughan, Secretary of the American Philosophical Society. It is preserved in the archives of the Society. The *North America Sylva*, such was the English title, was published in Philadelphia in 1817, but the text and the magnificent plates were printed in Paris. It was acknowledged at once as a fundamental work and went through three editions, the second seems to have been destroyed in a fire, the third, with additional annotations by J. Jay Smith, is dated Philadelphia, 1857.

The rest of Michaux's life was industrious and uneventful. In 1822 he bought in Vauréal, on the bank of the River Oise, a large plot of land on which he built a comfortable house and a small pavilion for his gardener. The buildings are still standing, practically unmodified. The property passed through several owners during the last fifty years and very little is left of the experimental garden laid out by Michaux, but many of the trees have survived. He married at a date which has not been ascertained, between 1829 and 1834. In 1829 he accepted the administration of the Harcourt estate, near Bernay, for the Société d'Agriculture de Paris to which it had been bequeathed by a rich lover of plants and trees, Louis-Gervais Delamarre. Most of the thousand acres of the domain consisted of barrens and brushwood, a

poor place to exploit and draw profit from, an ideal place in which to experiment for the man who thirty years earlier had been sent to America with the mission to discover "les essences forestières susceptibles d'être naturalisées dans des terrains incultes où les essences indigènes refusent de croître." For the next twenty-five years François-André divided his time between Vauréal and Harcourt. He was an active member of the agricultural societies of the region and became "maire of Vauréal." We do not know much about his private life. According to local traditions he quarreled with several of his neighbors and with the village priest which did not prevent him from going to church on Sundays. He never actively engaged in politics, but there are good reasons to assume that he preserved a not too secret preference for republican institutions, under the different régimes through which he lived. He died on October 23, 1855. He had prepared his tomb, not in the village cemetery, but on his own land in a grove of exotic trees, most of them of American origin. There he is still resting under an unornamented slab; his wife who died in 1870 was buried next to him in a similar grave. After changing hands several times during the last hundred years Michaux's garden and house became the property of M. and Mme P. Gouffier who restored the tombs with a pious care for which all the friends of Michaux must be grateful.

On January 15, 1824, François-André Michaux presented to the American Philosophical Society of Philadelphia the manuscript diary of his father as a token of his gratitude for the support that his father and himself had received from the Society. He intended to do more for the country which had treated him so generously. On several occasions he had signified his ambition to foster the development of sylviculture in the United States. By his last will written a month and a half before he died, he bequeathed to the two American societies to which he belonged sums of money representing for the time a considerable amount. I have indicated elsewhere (*Proceedings* **89** (2), July 18, 1945) the impulse given by the Michaux legacy to the work of forest conservation in the United States. My colleague, Professor Jacob R. Schramm, has assumed the pleasant duty to treat more in detail and more expertly the subject. I may be permitted, however, to reproduce here, in the original French, the part of Michaux's will which deals with this generous gift.

Voulant reconnaitre les services le bon accueil et la cordiale hospitalité que mon père et moi ensemble et séparément avons reçu dans nos longs et souvent périlleux voyages, dans toute l'étendue des Etats-unis, comme une marque de ma vive reconnaissance et aussi contribuer dans ces pays à l'extension et aux progrès de l'agriculture et plus spécialement dans la sylviculture dans les Etats unis. Je donne et lègue à la Société philosophique américaine de Philadelphie dont j'ai l'honneur d'être membre la somme

FIG. 2. The house in Vauréal.

FIG. 3. The gardener's pavilion.

de douze mille dollars à 5.50 le dollar . . . 64800. Je donne et lègue à la société d'agriculture et des arts de l'état de Massachussetts, Boston, dont j'ai l'honneur d'être membre la somme de huit mille dollars, ci 43 200. Ces deux sommes forment ensemble celle de *cent huit mille francs* ou encore vingt mille dollars. Je donne et lègue la nue propriété aux deux susdites sociétés et l'usufruit sa vie durant à mon épouse susnommée.

IV. TWO OUTSTANDING AMATEURS: CHATEAUBRIAND AND JEFFERSON

This brief historical survey is at the same time too long and very incomplete. To make it less imperfect one would have to undertake long and careful investigations requiring many years and much fieldwork. We believe however that the result of these inquiries would only corroborate our remarks on the importance both scientific and

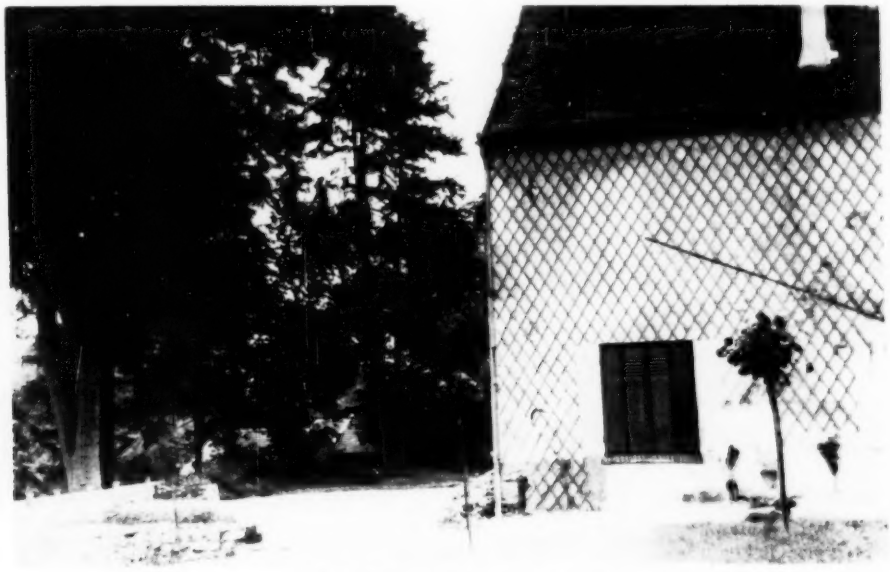

FIG. 4. The grave under the trees.

economic of the botanical exchanges between the ancient and the new world in the eighteenth century and during the first half of nineteenth. In the United States much material could probably be discovered in the archives of the Gray Arboretum at Harvard, in New Haven, in the proceedings of the New York Lyceum, and in the papers of the numerous agricultural societies found in the United States even before the end of the eighteenth century. It would also be desirable to examine from this special point of view the relations of the French travelers such as Chastellux, La Rochefoucauld-Liancourt, and Volney. In Philadelphia, the du Simetière papers preserved in the Library Company would provide interesting leads. Literature proper should not be neglected. In the many poems on gardens and agriculture written in France during the last decades of the eighteenth century, exotic plants are regularly mentioned. As an example between dozens of others, I shall only mention the poem of Adrian Lezay-Marnésia, *Les Paysages ou essais sur la nature champêtre*. The second edition, published in 1800, contains the picturesque description of an exotic garden established by comte d'Henezel on his Beaujeu estate, in Franche-Comté. The experiment succeeded so well that Henezel had planned to bring from America birds which would have found appropriate food in the groves.

Rafinesque and Palisot de Beauvois have already attracted the attention of American botanists. Less known is Pierre-Jean Turpin who studied the flora of Santo Domingo and came to the United States in 1800. A letter from him to the Secretary of the American Philosophical Society, dated from Paris, June 21, 1821, accompanying a copy of his *Iconographie végétale*, recalls that during his stay in Philadelphia he had been very cordially received by "la plupart des membres de cette illustre société." Many other names should be mentioned here; some will be found in the papers written by my colleagues; I shall cite only Victor Jacquemont whose travels in the United States are discussed in a volume soon to be published by the Paris Muséum. But it would be inexcusable to bring this sketch to a close without paying a special tribute to two amateur botanists who, the first one in France, the second both in France and in America, played an outstanding part in the diffusion of exotic plants.

The contribution of Chateaubriand to an exact knowledge of the American flora is probably rather thin. At most he was an amateur, but his contribution to the vogue of American plants can hardly be measured. He was not entirely devoid of scientific pretension. He had spent long hours in the Jardin du Roi, studying the plants and the trees under the expert guidance of M. de Malesherbes. He visited Bartram's gardens in Philadelphia and we may even suspect that he obtained most of his exact and poetical data from the book of William Bartram. He published in his *Voyage en Amérique* the notes accumulated during his American excursion, as well as those he had gathered from the published works of his predecessors. The extent of his travels in America is a moot question, but it is certain that he revealed to the French public and to a certain extent to the American public the unique beauty of the American forest in his "roman poème" *Atala*. Later in life, he enjoyed filling his small park at La Vallée-aux-Loups with American trees, some of which can still be seen. He was delighted when he happened to recognize them in the gardens planted by sea captains around Saint-Malo. He even drew up a list of the castles in the vicinity whose owners cultivated with loving care exotic trees and plants (*Mémoires d'Outre-Tombe*, Levaillant ed., Book V, ch. 1). From the many passages dealing with American trees in his works, I shall quote only one, chosen not at random but because it recalls the letter in which the French botanist Jutel was inquiring from Locke, about the "arbre tulippe," one hundred and fifty years earlier. Chateaubriand's notation is dated September, 1833. As he had done on several occasions on his way to Rome, he entered Italy through Milan, and as he was wont to do he rejoiced in finding there the stately trees he had admired in America:

> Je revis en entrant à Milan, la magnifique allée de tulipiers dont personne ne parle; les voyageurs les prennent apparemment pour des platanes. Je réclame contre ce silence en faveur de mes ouvrages; c'est bien le moins que l'Amérique donne des ombrages à l'Italie (*Id.*, Book 40, ch. 3).

In the history of scientific exchanges between America and foreign countries, Thomas Jefferson occupies a place of his own and is not second even to Benjamin Franklin. No particular study has been made of the friendly correspondence he kept up with French men of science, and one hesitates to treat from the point of view of a single nation of the influence of a man who recognized no frontier in the realm of the intellect. Thirty years ago I published the letters which passed between him and Madame de Tessé (*Trois amitiés françaises de*

Jefferson, Paris, 1927). All the necessary material for a broader study in the field of botany and agriculture has been gathered and published with remarkable industry and authority by Professor Edwin Morris Betts, in two books respectively entitled: *Thomas Jefferson's Farm Book, with commentary and relevant extracts from his other writings*, American Philosophical Society, 1953, and *Thomas Jefferson's Garden Book, 1766–1824*, American Philosophical Society, 1944. One must chiefly refer to the latter work which presents a day by day picture of Jefferson's botanical interests. He made the first entries in his garden book in 1766, when he was only twenty-three, and kept it regularly until two years before he died. It appears from it and from other sources that his interest in foreign productions was stimulated in 1773 by the coming of the Italian Mazzei who brought over with him grape vine cuts and seeds from Provence and Tuscany.

Jefferson's mission to France gave him an opportunity to form personal connections with the French botanists and amateur horticulturists. He arrived in Paris on August 6, 1784, and early in September explored the nurseries at Versailles. The following year he visited Malesherbes's garden whose owner he deemed the man in France who knew most about trees (October 28, 1785). This is an indication to remember. On the following year he sent long lists of desiderata to David Ramsay in Charleston and William Bartram in Philadelphia, asking them to procure seeds and plants for friends he "wanted very much to oblige." He started with Madame d'Houdetot and Madame de Tessé a correspondence in which botany occupied as much place as politics and friendly comments, and kept it up after his return to America. In the *Notes on Virginia*, the French edition of which was published in Paris in 1787, he had given a long catalogue of the native plants growing in his State, supplementing the Linnaean nomenclature with the names commonly used in Virginia. The list is very precious for it explains the confusion resulting from some of the popular appellations. One may note for instance that in Virginia the *Liriodendron tulipifera* was and still is called "poplar." After his return to America, Jefferson established with his "old friend Thouin" a regular system of botanical exchanges, kept up to the death of Thouin. Very few of the letters which must have passed between them have been found. It is very likely that some have been preserved in the archives of the Paris Muséum. It would be no less interesting to unearth Jefferson's correspondence with Honoré Jullien which seems to have been even more regular. There is no doubt at any rate that for more than thirty-five years Jefferson kept up with the Muséum the exchanges inaugurated while he was in Paris.

A striking illustration of the price he attached to these friendly gifts is found in a letter he wrote to John P. Emmet less than two months before he died, on May 12, 1826. The box of seeds he used to receive regularly every year from Thouin had been misdirected and sent by mistake to Madison. Jefferson most emphatically and somewhat peevishly protested and claimed for himself the precious parcel:

> By a letter from Mr. Madison I now learn that Thouin has been dead some time, that his successor sends the box annually to him as President of the Agricultural society of Albemarle, that such a box is now arrived at N. York, of which he has notified Secretary Barbour, his successor, to him I have written it's consignment to us, and the sooner the better . . . (Betts, p. 621).

CONCLUSION

In the course of this communication I have alluded altogether too briefly to the philosophical creed of our predecessors. It did not consist in a set of lofty principles; it was a living faith finding its application in everyday life and in the most ordinary circumstances. There was no need of a charter to organize and codify the institutions of the "republic of letters." An international fraternity, common ideals and tasks united the scientists of all the civilized nations. This oecumenical spirit reached its apogee during the eighteenth century but did not disappear with the century. The natural philosophers and the natural scientists were eager to promote the advancement and the diffusion of knowledge, but their ambition was to place knowledge in the service of mankind.

The object of the botanical exchanges was not simply the sharing of theories and ideas. Seeds and plants that passed from one country to another had more than a symbolic or token value. In many instances they had a direct bearing on the lives of the people at large. New plants provided new remedies, more abundant food, new supplies of precious material for the industry, and new ornaments for the gardens. Many of the species thus transplanted and acclimatized have become integral parts of our natural surroundings. They are not to be looked for in botanical gardens; they grow in our woods, in the fields, and along the

roadside. They have become so familiar to us that we do not recognize their foreign origin; yet they testify to the solidarity which for centuries has united the various members of the human family.

This has been said very often and very eloquently but never more strikingly perhaps than by a statesman whose interest extended from the common vegetables grown in the kitchen garden to the stately trees of the French parks and the American forests. As an illustration and in conclusion I may be permitted perhaps to turn again to Thomas Jefferson.

On February 19, 1809, a few weeks before the end of his second term as President of the United States, Jefferson wrote to his friend John Hollins a long letter in which he gave several instances of the way international exchanges were carried out between scientists and scientific bodies. The first bears upon a very humble vegetable, but probably there is no humble plant for the botanist and the good gardener. It was simply a new variety of a "perennial succory" the seeds of which had been obtained in France by Arthur Young, then sent to Virginia by the Board of Agriculture of London, presented to Washington and by Washington to Jefferson, and no doubt distributed by Jefferson to his Virginia neighbors.

Much more significant was another example quoted by Jefferson in the same letter. He had invented a plough which "on trial with a graduated instrument did equal work with half the force of the best ploughs then used in France." He communicated his invention to the Agricultural Society of Paris of which he was a member. It happened that the Duke of Bedford was also a member of the same society, but "although the two nations were then at war," the French hastened to send one of the model ploughs they had manufactured "to be made use of in England," thinking that "it would be a benefit to mankind to communicate it."

Jefferson had too much experience of politics and of the "passions" and "malice" that sway non-enlightened individuals, not to realize that this "liberal diffusion of the blessings" of science did not meet with unanimous approval. But he knew that it was a faith held out and practiced by scientific bodies the world over and, with his usual felicity of expression, he drew from these instances the following lesson well worth remembering even today:

> I mention these things, to show the nature of the correspondence with which is carried on between societies instituted for the benevolent purpose of communicating to all parts of the world whatever useful is discovered by any one of them. These societies are always in peace, however their nations may be at war. Like the republic of letters, they form a great fraternity spreading over the whole earth, and their correspondence is never interrupted by any civilized nation.

ADDENDA

I. RHUS TOXICODENDRON

The scientific curiosity aroused by the noxious plant should not have puzzled the author of this article. Valmont-Bomare has devoted a long article to the *Rhus toxicodendron ou Arbre à la gale*, Linn. 381, in his *Dictionnaire raisonné et universel d'histoire naturelle* (4th edition, **14**: 365, Lyon, 1791). In his description of the symptoms caused by a mere contact with the leaves, he quotes Duhamel and Peter Kalm and refers to a communication published in the *Journal de Physique* **21**, supplément, 1782. He adds, however, that a Valenciennes physician had successfully used an extract of toxicodendron in the treatment of pityriasis and paralysis of the lower limbs. Quite recently I was told by a Philadelphia homeopathic M.D. that "poison ivy" was still used quite frequently as a very efficient remedy for asthma.

Apparently *Rhus toxicodendron* was successfully acclimatized. I found it advertised among the "Moyens arbres et arbustes pour Bosquets, Massifs et Palissades," in the *Catalogue d'arbres* printed in Orléans, in 1784, by Bruzeau fils aîné, Agriculteur à la Croix St. Marceau. Some twenty years ago it was still listed in the catalogue of a nurseryman established near Tours, as "lierre du Canada," a plant to be used in borders, without any caveat as to the real danger it constitutes. Valmont-Bomare concluded his article with a dreary description of the aspect of the shrub in winter: "Il y a une espèce de toxicodendron au Jardin du Roi dans les plates bandes en forme de buisson. Cet arbuste depouillé de ses feuilles, noir, triste, d'une forme peu agréable, ramassée, desséchée, semble porter un caractère de reprobation." Beyond any doubt it was a very undesirable importation.

II. FRANKLIN AND THE FRENCH BOTANIST

Documents recently acquired by the American Philosophical Society indicate that Franklin actively engaged in botanical exchanges with French

flora
america septentrionalis
By john Reinhold forster
London 1771

Cette liste a eté faite pour remettre a Mr de Malhesherbes qui doit la donner a Mr francklin qui luy adressera en droiture ces graines.

chionanthus virg. fringe tree.
Dianthera americ.—Bastard hedge—hyssop.
Diodia virginiana
houstonia caerulea
catesbaea spinosa
Ptelea viscosa—
Dorstenia Louisian.
azalea nudiflora
triosteum perfoliatum
angustifolium

Laurus
Sassafras
Rhododendron maximum—Rose bay
Spiraea trifoliata—Trifoliated
Bignonia semper virens—yellow

Fagus pumila—chinquapin
Liquidambar—Sweet gum Trée
liriodendron—Tulip-trée.

magnolia	grandiflora	magnolia great
	glauca	swamp
	acuminata	acuminated
	glabra	Smooth
	tri petala	umbrella
annona	muricata	Custard apple
	reticulata	reticulated
	glabra	Smooth
	triloba	trilobated
Stewardia	malacodendron	
Robinia	speudacacia (*sic*)	locust trée uniflorous
	hispida	
carpinus	betulus	horn beam
	ostria	iron wood
liquidambar	styraciflua	Sweet gum-trée
	o. peregrina	
cupressus	disticha	
	thyoides	
acer	rubrum	Maple red
	saccharinum	sugar
	pennsylvanicum	
	negundo	ash leaved
nissa	aquatica	Tupelo Tree

botanists much earlier than was suspected. From the list published here we see that as early in 1771 he acted as an intermediary between the English botanist John Reinhold Forster and M. de Malesherbes. He was in a position to have sent directly to France (*en droiture*) plants and seeds he had probably obtained from Bartram. In another document dated 1785 and soon to be published, it appears that both Franklin and Jefferson were consulted as to the practical value of the different kinds of wood which could be purchased in the United States. Here, as in so many other instances, Franklin acted as a willing intermediary between the two countries.

FRENCH BOTANISTS AND THE FLORA OF THE NORTHEASTERN UNITED STATES: J. G. MILBERT AND ELIAS DURAND *

WILLIAM J. ROBBINS

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BEFORE I discuss the botanists with whom I am concerned, you will be interested to know that two manuscripts, the result of ten years of research by the late M. Léon Rey, have been generously entrusted by Madame Rey to Dr. Gilbert Chinard, a member of the American Philosophical Society of Philadelphia. One of these deals with the American missions of the two Michaux and includes an extensive study of the Royal nurseries from the beginning of the eighteenth century until their dissolution about 1830, as well as much information from the French archives on the mission of André Michaux to America and the plants he sent back. The second contains the extensive correspondence of St. John de Crèvecœur with Maréchal de Castries and is rich in botanical information.

The manuscripts are being edited and supplemented by Professor Chinard, and it is expected that they will be published by the American Philosophical Society of which François-André Michaux was a member and benefactor. These two manuscripts, which have been tentatively accepted by the Publications Committee of the American Philosophical Society—Dr. J. Schramm, Chairman of the Committee, is participating in this meeting—will be important contributions to the objectives of this conference.

Six days after landing on San Salvador, Christopher Columbus wrote, "All the trees are as unlike ours as day is to night, and so are the fruits and also the plants." In spite of these observations he does not appear to have carried back to Europe much cargo from his first voyage, but from the second he sent home a considerable collection of vegetables of the New World. Christopher Columbus might, therefore, be regarded as the first collector of plants of America.

Like those of Columbus, the explorations which followed were made for the most part in tropical areas and, as a consequence, plants from tropical America became known to Europeans earlier than those from the temperate regions.

In the early botanical explorations of North America, especially in Canada and the northeastern United States, Frenchmen played an important role. André Thévet published in 1557–1558 *Les singularitez de la France antarctique, autrement nommé Amérique*. . . . Although mostly concerned with Brazil and other parts of South America, he wrote of Florida and Canada and described maize, tobacco, pumpkin, gourds, and a few plants not in cultivation. Jean Nicolet, a trader, is reputed to be the first European who saw the prairies of Illinois.

It is not possible within the limits of a brief paper to discuss adequately the contributions made by Frenchmen to our knowledge of the flora of northeastern North America, or even to list the names of all those who in one way or another played a role in the exchange of plants between Europe and North America or explored the botanical riches of that great continent. I can only call your attention to a few of those to whom we are all so deeply indebted.

One of these is Father Sagard who published in 1635 his great *Histoire du Canada et Voyages que les Frères Mineurs Recollects y ont Faicts pour la Conuersion des Infidelles*, in which part of Chapter 5 of the third book is devoted to the fruits, plants, and trees of the country. Some say this work is characterized by extreme inaccuracy and credulity. However, although it is difficult, if not impossible, to identify all the plants he mentions, his comments on them and their uses by the Indians are written in a most interesting fashion and on what seems to be personal experience.

He begins by saying that it is impossible that those who undertake to describe the things in a large country should not deceive themselves sometimes as was done by those who have said there are no cedars and no grapes in America. He reports, with what I interpret to be extreme regret,

* Presented at the Centennial Observation of the death of François-André Michaux at the Colloque International No. 63, Paris, September 11–14, 1956. The whole report of this colloquim is found in the book, *Les Botanistes Français en Amérique du Nord*, published by Centre National de la Recherche Scientifique, 360 pp., 34 pl., Paris, 1957.

that the grapes of the Huron country would make a good, strong wine if the savages had the ingenuity to cultivate and convert them, but lacking sufficient knowledge they are content to eat them without making wine of them. It is strange to me that the North American Indians did not make a fermented drink since the production of alcoholic beverages was so general amongst primitive peoples in other parts of the world.

Sagard tells of eating a small white "onion" with leaves like the Lily of the Valley[1] with a little salt and pepper but "without bread, without oil and without vinegar." He mentions roses, lilies, and various fruits and roots, but the most beautiful plant in his opinion was one which the Hurons called shell-of-the-turtle, the leaf of which he says—except for color—resembles the shell of the homard; closed at the bottom and hollow like a cup, it furnished, in case of need, a drink of dew which collected in it each summer morning. I assume this plant which he admired so much to have been the pitcher plant, *Sarracenia*.

He described how the Indians obtained oil from seeds of the sunflower and asks, "How is it that a savage people is able to develop the ingenuity to render an oil of which we are ignorant, unless it is by the aid of Divine Providence which gives to each of us the means for our preservation and without which, having no instruction, people would remain miserable like brutes themselves." I commend Sagard to you as a true Gaul and a keen observer of plants, though no botanist.

The first work relating specifically to plants north of Mexico was published in 1635 by Jacques Philippi Cornuti, a Parisian physician, less than thirty-five years after the first permanent settlement in Canada. The book is illustrated by excellent drawings and most of the species illustrated are readily recognizable. *Asarum canadense* is a good example; it was given almost the same name, *Asaron canadense*, by which it is known today. Though not all the plants included came from the New World, a number of plants from northeastern America were described and figured for the first time.

Cornuti was probably never in Canada, and it is not known who supplied him with the plants he described and figured. It was not Michel Sarrasin, the first Canadian resident botanist for whom the pitcher plant, *Sarracenia*, is named, because Sarrasin was born twenty-five years after Cornuti's book was published.[2]

Pierre François Xavier de Charlevois first came to Quebec in 1705 and published in 1744 *Histoire de la Nouvelle France* which contains observations on plants. François Jean de Chastellux in *Voyages dans l'Amérique Septentrionale dans les années 1780–1782* and Claude C. Robin in *Nouveau voyage dans l'Amérique Septentrionale en 1781* included observations on plants. Constantin François Chassebœuf Boisgirais, a friend and correspondent of Benjamin Franklin, came to the United States in 1795, visited Pennsylvania, Virginia, Kentucky, and Louisiana, and wrote *Tableau de climat et du sol des Etats Unis d'Amérique* in 1799.

Reference might also be made to François Barbé-Marbois who came to Philadelphia in 1779 as First Secretary of the Legation when the Chevalier de la Luzerne was the new French Minister. Barbé-Marbois was a student of botany and much interested in the strange plants, nut trees, and flowering trees of America. In an incredibly short time he nursed the vanished legation gardens back and planted magnolias, sassafras, tulip trees, new vines and flowers. He dug and planted with his own hands and took pleasure "in seeing the progress of his family" (of plants). For him the legation garden took the place of concerts, opera, comedies, hunting, promenades, and picture galleries.

However, the most famous and the best known botanically of French visitors to the United States were André Michaux and his son François. The contributions of André Michaux and his son to the broadening and deepening of cordial relations between France and the United States of America are well known. André arrived in the United States in 1785 with introductions to George Washington and at once proceeded to establish a garden in the vicinity of New York.

He came to the United States through the efforts of J. Michel Guillaume St. John de Crèvecœur who had arrived in the English Colonies by way of Canada in 1760; settled in Orange County, New York, and after the American Revolution was French consul in New York for a time. Crèvecœur was deeply interested in agriculture on

[1] I judge this to be wild leek, *Allium tricoccum*.

[2] I have learned from Professor Jacques Rousseau, Director of the Montreal Botanical Garden, that the North American plants figured in Cornuti's book were drawn from plants grown in the Paris garden of the School of Medicine and may have been sent by Lewis Herbert, the first colonist in Canada and a former apothecary. Since Cornuti has a word of praise in his introduction for an illustrious navigator, Professor Rousseau assumes that Samuel de Champlain, who was interested in botany, may have had a part in supplying the plants. Professor Rousseau's further researches will be awaited with much interest.

which he wrote extensively and charmingly. He introduced a number of plants into America, corresponded with noted farmers of his time, including George Washington and Thomas Jefferson, and assisted in laying out a botanical garden at New Haven, Connecticut. Crèvecœur's influence on the botanical relationships between France and the United States was great, and I believe has never been fully evaluated.

Because Michaux was not a citizen, it was necessary for him to be granted permission to hold land and the Legislature of the State of New Jersey passed an act on March 3, 1786, enabling André Michaux, "botanist to his Most Christian Majesty," to purchase land to be used for the sole purposes of a botanical garden. In the preamble of the Act, it was stated that Michaux "has full powers to draw from France, at the King's expense, any tree, plant or vegetable that may be wanting in this Country" (U.S.A.). Michaux planned to establish a garden of about thirty acres "in order to make useful experiments with respect to agriculture and gardening, and to make a depository not only of French and American plants, but of all other productions of the world which may be drawn from the King's garden at Paris."

He purchased thirty acres across the Hudson River, about opposite 60th Street in New York City, for $750, and on May 12, 1786, sent M. Cuvilliers a letter which gave an accounting of expenses and was accompanied by a plan showing the location of the garden bordering on Cromakill Creek, a branch of the Hackensack River, and traversed by the Hackensack Turnpike. It was located four miles by water from New York City to the Weehook (Weehawken) Ferry and thence two miles by land. Plans of the garden itself and the house built on the grounds were also furnished.

Michaux was accompanied to the United States by Pierre Paul Saunier, journeyman gardener, aged seventeen years, who was entrusted with the care of the garden. The garden, popularly known as "The Frenchmen's Garden" (probably because of Saunier rather than Michaux, who remained at New York for one year only), became a fashionable recreation spot for early residents of New York. Paul Saunier remained in the United States until his death in 1818. During these thirty-three years he purchased several parcels of land in the vicinity of the original Michaux Garden, and his children married in the community.[3]

Paul became a garden oracle, giving instruction on propagation, budding, grafting, and the care of fruit trees and ornamental plants as well as in the principles of classification of Jussieu in which he had been well grounded through his early training at the Jardin des Plantes. He is credited with the introduction into the United States of the Lombardy poplar[4] and with bringing into cultivation *Castenea pumila*, the chinquapin, as well as other meritorious plants. His hospitality was enjoyed by Pursh, Douglas, Bradbury and other foreign naturalists who came to the United States.

Dr. H. H. Rusby, for many years on the staff of the New York Botanical Garden, married a great-granddaughter of Paul Saunier and describes a visit made in 1884, with two granddaughters of Paul, to the site of the Michaux Garden.

When André Michaux returned to France in 1796 the garden and commissions were transferred to Saunier, and afterwards to his oldest son, Michel, who continued to make shipments of plants and seeds nearly to the time of his death. One of the granddaughters described to Rusby the appearance of the wagons with their great stacks of boxes looking from a distance like loads of hay. The orders steadily decreased until they ceased in the 1830's with one final shipment of three small boxes.

François Michaux never spoke of Paul but with respect and affection, as may be judged from the following quotation:

> Paul was so exclusive in his attentions to his avocations that hardly any other subject than trees and plants ever found entrance into his mind. To the day of his death, he considered his little circumscribed residence as still the property of his royal master; and ignorant of the vicissitudes of political

[3] I have been in correspondence with Miss Ruth Saunier and Pierre Paul Saunier, great-great-grandchildren of Paul Saunier, who live in the United States. Durand gives Saunier's age as seventeen. Other sources indicate that he was thirty-four when he came to the United States.

[4] Saunier may have introduced the Lombardy poplar to the New York area. He was not the first to bring it to the United States. Ethel Armes in *Nancy Shippen. Her Journal Book* (J. B. Lippincott, 1935) says that the first weeping willows and the first Lombardy poplars in the colonies grew at Woodlands, the country seat of Andrew Hamilton on the west bank of the Schuylkill River near Philadelphia. The date was probably about 1745.

revolution, reluctantly gave credence to the fact of the decapitation of his bountiful but unfortunate King. Paul now lies in the Hackensack churchyard; his tombsone records not half his excellence.

Sometimes in our absorption with the great we may forget those more humble like Paul Saunier, journeyman gardener, whose influence was far greater than his position in life might suggest.

Another Frenchman to whom the State of New Jersey granted the right to hold real estate was the Count de Survilliers (Joseph Bonaparte). He purchased in 1816 an estate of 1,000 acres (later increased to some 1,800 acres) near Bordentown, New Jersey, about sixty miles from New York City. He remodeled the house already on the estate, added a number of smaller buildings and established a large garden, including many rare plants, in which his daughter, Princess Zénaide, tended her flowers. If we accept the account in the novel *The Tontine* by Thomas B. Costain, as approaching truth, Joseph was especially interested in trees. We know that he accompanied the Philadelphia botanist and pharmacist, Elias Durand, on botanical excursions. All that remains of Joseph Bonaparte's gardens is one house and perhaps some of the trees which he planted and loved so much.

Before I close these fragmentary remarks, I should like to speak of two additional Frenchmen, Jacques Gérard Milbert and Elias Durand.

Jacques Gérard Milbert was born in 1766 in Paris where he died in 1840. He was professor of design at the École des Mines and in 1800 accompanied the expedition to southern lands commanded by Captain Baudin. Illness constrained him to stop at the Île-de-France for two years. As a result of his observations there he published in 1812 *Voyage pittoresque à l'Île-de-France, au Cap de Bonne-Espérance, et à l'Île de Ténériffe* in two volumes and an atlas of which the illustrations were in part engraved by the author.

We are interested in Milbert because he came to America in 1815 charged by Hyde de Neuville, the French Minister to the United States, to investigate natural history. He remained in the United States for seven years, during which time he traveled extensively, met many of the prominent men of the day, collected objects of natural history which he sent back to France, made sketches of parts of the country he visited, and observations on geology, geography, local history, customs, and conditions. He suffered illness and had difficulty in obtaining sufficient funds to support his explorations.

The record of his stay in America is found in two volumes and an atlas, entitled *Itinéraire pittoresque du fleuve Hudson et des parties latérales de l'Amérique du Nord, d'après les dessins originaux pris sur les lieux*,[5] published 1827–1829.

He collected and sent to Paris 25 cases or barrels of seeds of 300 species of plants, herbarium specimens and 600 living trees, as well as mammals, birds, reptiles, fishes, insects, geological specimens, and other items of natural history. His shipments included a live eagle, a live bison, a live elk, live turtles, and a live "crocodile"; the latter, dead on arrival. In a report by Louis Cordier, De Jussieu, and Brongniart, to His Excellency, the Minister of State in the Department of Interior, mention is made of new "species" of oak, elm, maple, poplar, sumac, walnut, chestnut, thorn, pine, cedar, juniper, spruce, fir, larch, and cypress sent by Milbert.

His interest in collecting trees for France was in part, at least, because of his conviction of the need for the conservation of forests and reforestation. In the introduction to his book, Milbert makes a moving plea for the preservation of forests, the destruction of which he noted around New York and other large cities he visited in the United States. He was concerned with forest conservation because he had seen sections of France, once covered with forests and filled with game, where deforestation had resulted in the erosion of pastures, leaving bare rock; had eliminated trees which had acted as windbreaks and protected fruit trees and other crops in the neighboring plains from the fury of the winds; had changed beautiful rivers into those which were raging torrents at some periods of the year and dry rock-filled beds at others, and limited the water which once had irrigated fertile lowlands. This attitude on forest conservation is so modern that it is hard to realize that it was expressed nearly 150 years ago.

Milbert gave special attention to plants other than trees which might be useful in France. For example, he sent back the dog bane (*Apocynum*) because the pappus of the seeds resembled cotton. This reminds me that the similar pappus of the seeds of milkweed (*Asclepias*) was gathered in the last war and used for filling life belts. He sent a desirable variety of potato which he located in Philadelphia, and *Urtica* from which, he reported, cheap rope and a superior oakum were prepared.

[5] Miss Constance D. Sherman of the American Museum of Natural History has translated this work into English, and it should soon be available in published form.

He sent a plant which he called "red top," used, according to Milbert, in the state of Connecticut to make hats rivaling those of Livornice (Leghorn) and selling for 500 francs. I suspect the fantastic price he gives, which at the time was the equivalent of $100.00, must be an error. The only plant I know which is native to the United States and called "red top" is a grass, *Triodia flava*. *Agrostis stolonifera* introduced from Europe also has the common name "red top." In any event, I have been unable to find any information on the making of hats in Connecticut from an indigenous plant in the early part of the nineteenth century, though hat-making is today one of the important industries of that state.

What manner of man was Milbert? He spent nearly eight years away from his family and his beloved France and still requested, on his return, support for another expedition to the United States. His relations with his family must have been good. A son transmitted to the French authorities Milbert's request for a gun (which he received a year later) and asked more than once for additional funds for his father's work. Milbert himself records his pleasure at the prospect of returning to France and to his family and friends. What made him endure the sacrifices necessary to accomplish his mission? Through the courtesy of Mlle M. G. Madier of the Museum of Natural History, I have learned that there are in the Archives Nationales twelve letters written in 1820 by Milbert from New York to the Museum of Paris, seventy-four pages of letters between 1819 and 1823 addressed to him or written on his subject, and a catalog of about forty-five pages listing the shipment of various items during his sojourn in the United States. These I have not yet had the opportunity to examine in detail.

His books, reports, and correspondence, so far as I have seen them, reveal a man of great patriotism, restless energy, an insatiable and all-embracing curiosity with considerable artistic ability and, as Georges Cuvier said, "une persévérance inouie." I would surmise that he was ambitious, anxious to impress the distinguished professors and others in France to whom he sent his specimens, and made his reports, and I would assume also that he had an engaging and attractive personality. In other words, Milbert must have been a man of parts, one of those individuals whose combination of characteristics makes them indispensable units in man's quest for knowledge.

Elias Durand (Élie Magloire Durand) was born at Mayence, France, in 1794 and died in Philadelphia, in 1873. He was the youngest of fourteen children, though he himself had but two, a daughter and a son—only one, the son, surviving.

His father was a recorder of deeds. At the age of fourteen, Elias began a period of four years of study under M. Chevalier, a pharmacist of Mayence. In the fall of 1812 he arrived in Paris where he attended lectures by Thinard, Gay-Lussac, Lefévre, and Gissault. He received a commission as assistant pharmacist, presented his commission to Field Marshal Kellerman, and joined the army at Magdeburg. He participated in most of the battles which followed, including the bloody one of Leipzig, during which it is said he gathered a specimen of *Menyanthes trifoliata*, thus evidencing under extraordinary circumstances the interest in botany which later dominated his life. At the battle of Hanau he was one of about a dozen who survived unharmed out of some hundreds killed and wounded in an attempt to cross a stream. He was captured by the enemy; however, the officer-in-charge, taking pity on the few left from so much destruction, freed Durand after a short detention.

On the downfall of Napoleon, Durand resigned his commission and was employed in a drug store in Nantes where he directed chemical manipulations in the apothecaries' garden and lectured to students on medical botany. On the return of Napoleon he at once rejoined the army and served during the celebrated one hundred days as one of the National Guards. After Napoleon's final defeat Durand, as a Bonapartist, found conditions uncomfortable in France and emigrated, in June, 1816, to the United States where, except for brief visits to France, he spent the balance of his life.

Durand landed in the United States at New York; he proceeded to Boston from where, after a few months of employment as superintendent in a chemical laboratory, he removed to Philadelphia. There he found a similar position which after a short period he abandoned in order to devote himself exclusively to pharmacy. He became chief clerk in the establishment of Mr. Ducatel of Baltimore, whose daughter he eventually married. When Ducatel retired in 1823 leaving the business to his son, Durand became a partner but at the end of the year he decided to open his own store in Philadelphia which he did in 1825, after a short trip to France.

Although his store ultimately became a most distinguished pharmaceutical establishment and Durand contributed many publications on pharmacy, his major interest was botany, to which he

devoted all his time after his retirement at the age of fifty-eight. His store was a gathering place for botanists. He financed many botanical travelers, made extended explorations in Virginia and the mountains of Pennsylvania, as well as shorter excursions with Joseph Bonaparte and others. He was elected a member of the American Philosophical Society and the Academy of Natural Sciences of Philadelphia.

According to Harshberger, a gentleman of means gave the young Durand, when he left France, funds with which to collect in North America plants which were little known and in great demand. Durand arranged with Rafinesque [6] to furnish the required collection. Rafinesque, always in need of money, contrived to be paid in advance, and on the very day of the sailing of the vessel which was to take the specimens to France, Rafinesque appeared with bundles of worthless rubbish. Durand was highly mortified at being obliged to send such poor material and determined to make amends by making collections himself. Within a few years he sent to his friend a satisfactory collection of plants.

This experience doubtless accounted for his interest, in part, in herbaria; he himself collected and obtained specimens from other notable collectors of his day. It doubtless influenced also his judgment of Rafinesque. Durand paid $45.00 for the herbarium of Rafinesque in 1843 or 1844, after the death of the latter. It had been stored in a stable garret for three years and its condition was described by Durand as "truly a dunghill." He removed it to a cellar and for a winter was occupied by candle light in separating the good from the rotten.

In a letter to Charles Wilkins Short, written in 1856 about his experience with this herbarium, Durand gives his opinion as follows: "Rafinesque's own specimens were trash, incomplete, good for nothing; he was the worst collector I ever knew." Most of Rafinesque's own specimens were discarded by Durand. Agnes Chase found no Rafinesque specimens among the grasses in the Herbier Durand at the Jardin des Plantes and Pennell found only eight out of sixty species of *Scrophulariaceae*.

Durand was an assiduous collector. He sold some 10,000 species to "a wealthy young gentleman of (Columbus) Ohio"; he added many desiderata to the herbarium of the Academy of Natural Sciences of Philadelphia, he sent a collection to the friend in France who gave him a commission when Durand first came to the United States and he presented a herbarium of medicinal plants to the College of Pharmacy of Philadelphia.

While on a visit to Europe in 1860 Durand formed the opinion that the herbarium of the Jardin des Plantes was deficient in species from North America. On his return to the United States he devoted much of his time to preparing a collection for that institution. He took the major portion to Paris in 1868 and conveyed the balance by bequest. The total amounted to about 15,000 species and forms the Herbier Durand. His botanical library is deposited in the Academy of Natural Sciences in Philadelphia. Thomas Meehan, in his obituary of Durand, refers to Durand's diary. I have failed to find it. It is not in the Library of the American Philosophical Society, of the Academy of Natural Sciences of Philadelphia, or in that of the Museum National d'Histoire Naturelle.

Although so long a resident of the United States, Durand remained a Frenchman at heart. When Napoleon's nephew had medals struck for those who served under the Emperor, none was more happy to receive the bronze token than Durand.

It is fitting that on occasion we should endeavor to bring back those of our colleagues who have gone before, even though our best efforts call forth but a shadow of the men who contributed so much to the science with which we ourselves are so deeply concerned. However, I must confess that at the moment I feel better acquainted with some of my botanical predecessors than I do with many of my present-day colleagues. For this I wish to express my very great and sincere appreciation to those responsible for arranging and conducting this conference.

[6] Constantine Samuel Rafinesque was born in Constantinople in 1784 of French parentage; came to Philadelphia in 1802; returned to Europe in 1805 with a collection of botanical specimens and returned to the United States in 1815 where he remained. His erratic genius has been the despair and inspiration of botanists.

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JAMESTOWN, 1607–1957

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(*Read April 26, 1957*)

On the sixth of May, 1607, three little vessels—the *Susan Constant*, the *Godspeed*, and the *Discovery*—their decks crowded with adventurers to the New World, entered the Chesapeake Bay. After spending a few days in exploring this great body of water, Captain Christopher Newport, who was in command, turned the fleet into the broad mouth of a river which he called the James. Finding a peninsula in the river which afforded a convenient landing place and was easy to defend from the Indians, he tied up to the shore and landed his men and supplies. The place he called Jamestown.

The founding of Virginia was an event of great importance to America, to England, and to the world. But it is misleading to claim that Jamestown was the birthplace of the United States. Our civilization resulted from the establishing on our shores of a number of beachheads of European civilization. It was the expanding of these beachheads—from Jamestown, New Amsterdam, Plymouth, Annapolis, South Carolina, Philadelphia—into the vast American west which created our nation.

But Jamestown was the first permanent beachhead. In 1585 Sir Walter Raleigh had sent over an expedition which planted a colony on Roanoke Island. But several years later when a relief party arrived the settlers had vanished. To this day we do not know what became of them. So this attempt ended in failure.

The fact that Jamestown did not suffer a like fate was of great importance in making good England's claim to a share of the continent of North America. It was with envy and alarm that the English had watched the expansion of Spain across the Atlantic, while they were excluded from the splendid prize. If they were to maintain their position as a great naval and mercantile nation the founding of colonies in America was imperative. But Spain claimed the entire continent, so the planting of the colony on the James might well have provoked her to war. It was the fear of the Spaniards which induced the colonists to make their settlement far up the river rather than near the coast. And for years they feared that some day they might see a fleet of warships heading up the James, with the dreaded Spanish ensign at the mastheads.

But no Spanish expedition appeared, and the English were left to expand their foothold. North America was to be chiefly English.

The success of the colony on the James led to the founding of other colonies. Friends of the Pilgrim founders of Plymouth wrote them: "Let it not be grievous to you that you have been but instruments to break the ice for others, the honor shall be yours till the world's end." If this is true, no small part of the honor must be shared with the men and women who founded Jamestown, for it was they who broke the ice for Plymouth. It is almost certain that had there been no Jamestown there would have been no Plymouth. It was to the London Company that John Robinson went for permission to settle in its territory and under its jurisdiction. And it was the pious Secretary of the Company, Sir Edwin Sandys, who pleaded with James I to give his assent. When adverse winds blew the *Mayflower* off her course and the Pilgrims sighted the sand bluffs of Cape Cod, they tacked about and sailed south, for they feared to remain in a place where they had no permission to settle. It was only when they fell among the shoals off Monomoy Point, that they were forced to return.

There might well have been a major migration of Puritans to Virginia in 1608 similar to the Great Exodus to Massachusetts which began in 1630. When the news that Jamestown had been firmly established reached London, many who resented "the advancement of absolute power then aimed at in England by Court and King," cast longing eyes on the colony. "Many worthy patriots, lords, knights, gentlemen, merchants, and others held consultation, and laid hold of this expectation of Virginia as a providence cast before them." But when the decision was finally made to establish a Bible commonwealth in America, the taking over of the charter of the Massachusetts Bay Company offered a more favorable opportunity.

As for Maryland, it was a rib taken out of the side of Virginia. That Virginia protested violently against the operation does not obscure the fact that Jamestown was the forerunner of Annapolis. And it was only when England's claim to a part of the continent had been backed by actual settlement that still other colonies were founded—the Jerseys, the Carolinas, Pennsylvania. Virginia has boasted of being the mother of Presidents; she could also boast of being the mother of colonies.

In fact she was the torchbearer of the English-speaking world. The little group who settled on the banks of the James were followed by millions, who carried the English flag, the English tongue, English law, English civilization to the remote corners of the globe. Jamestown was followed by Bermuda in 1612, Barbados in 1625, St. Christopher and Nevis in 1623. Today huge sections of the world, not only the United States, but Canada, Australia, New Zealand, are predominantly English in blood, language, and political institutions. One wonders whether, of the millions who went out from the mother country, or the thousands who are going out today, any save a handful know that it was Captain Newport who led the way.

It was at Jamestown that representative government was first established in the New World. The Virginia Assembly met in the little frame church on August 9, 1619. Sir George Yeardley, the Governor, presided, with the members of the Council on either side, the Speaker in front of him. "But forasmuch as men's affairs do little prosper when God's service is neglected, all the Burgesses took their places in the choir till a prayer was said by Mr. Buck, the minister." Then they retired to "the body of the church," and being "called in order and by name, . . . took the oath of supremacy."

But self-government had been won only after a bitter struggle. James I, in granting a charter to the London Company, purposely omitted any reference to an Assembly. He once said of the English Parliament: "I am surprised that my ancestors should have permitted such an institution to come into existence. I am a stranger, and found it here when I arrived, so I am obliged to put up with what I cannot get rid of."

So when he granted the charter he took pains to make the government of the colony dependent upon his will. It provided for a Council to be appointed by the King and to reside in England, who appointed another Council who were to go to Virginia and to whom was intrusted the immediate government, with administrative, legislative, and judicial powers.

This plan proved a failure. The Councillors quarreled with each other, there was little leadership, the people were subjected to "whipping . . . beating, and hanging." At one time Captain John Smith was the sole ruler. Finally a change was made in the government. The stork was sent to rule the frogs. A Governor was appointed with dictatorial powers, and armed with a code of cruel martial laws. Under two of the Governors—Dale and Argall—these laws were enforced with savage severity. The Virginia Assembly of 1624 testified that men "were put to sundry deaths as by hanging, shooting, and breaking upon the wheel." One man had a bodkin thrust through his tongue, and was chained to a tree and left to starve.

Great was the rejoicing in Virginia, then, when news came that Sir Edwin Sandys had secured new charters, and under their authority the London Company had granted the colony a Magna Carta, providing for the election of an Assembly. When this body met it was in effect an announcement to the world that Englishmen, in migrating to America, gave up none of the rights and privileges they had enjoyed at home.

One noble attempt made at Jamestown failed. When John Rolfe was delegated by the Governor to educate the captive Indian Princess Pocahontas in the Christian religion, and not only converted her but married her, it seemed but the beginning of the Christianizing of the savages. It was followed by liberal contributions in England for the founding of an Indian college at Henrico. The pious George Thorpe, a man of prominence in England, came to Virginia to live with and convert the Indians. But in the massacre of 1622 he was the first to be cut down. The college was wiped out.

It is fitting that the 350th anniversary of an event of such significance as the founding of Jamestown should be celebrated by the Jamestown Festival, with the State of Virginia and the Federal Government participating.

A prominent feature of the Festival is the reproduction of Newport's three ships. We do not know just what they were like, but it is assumed that they followed the usual lines of sea-going vessels of the day. In 1946 the late Griffith Baily Coale, of Stonington, Connecticut, who was commissioned to make a painting of the vessels, studied carefully the design of early seventeenth-cen-

FIG. 1. Tobacco cultivation as practiced in the early years of the Colony. Conjectural painting, authentic as to detail as established by research. Courtesy of the artist, Sidney E. King, and the National Park Service.

tury vessels before he set brush to canvas. His painting, which hangs in the Capitol at Richmond, has been followed closely by the builders of the replicas. There has been some confusion as to the names. The records of the London Company called them the *Sarah Constant*, the *Goodspeed*, and the *Discovery*, but Purchas, writing soon after, spoke of the *Susan Constant*, the *Godspeed*, and the *Discovery*. Since we have no further mention of the *Sarah Constant* and the *Goodspeed*, it is assumed that these names slipped into the records through clerical errors.

It is fortunate that at a time when so much interest is focused on Jamestown, the National Park Service, cooperating with the Association for the Preservation of Virginia Antiquities, should be making many archaeological discoveries which throw light on the daily life of these first Americans. The tower of the old brick church was still standing, a lone survivor of the day when Nathaniel Bacon and his men set the torch to the village to keep Governor William Berkeley from reoccupying it. The Memorial Church attached to the tower was erected over the original foundation in 1907 by the National Society of Colonial Dames of America. Within the walls is another foundation, on which the original frame structure seems to have rested.

Of great historic interest is the statehouse, a two-story building with the Council Chamber on the first floor, and the Assembly Room, or "long room" as it was called, above. It was from the windows of this room that the Burgesses looked down upon the exciting scene in June, 1676, when Bacon drew up his ragged men on the green and forced the enraged Governor Berkeley to give him a commission to lead an expedition against the Indians. It was rebuilt in 1685 and again burned in 1698. The foundation gives us the dimensions of the building, but we do not know just what the superstructure was like.

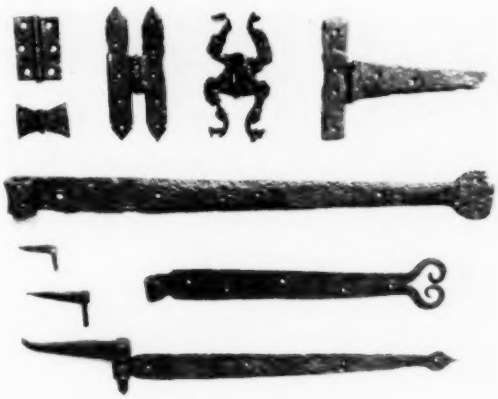

FIG. 2. A group of hinges and pintles. Courtesy of the National Park Service.

FIG. 3. A brick kiln, thought to be associated with Jamestown's first quarter century. Courtesy of the National Park Service.

Of the other foundations which have been uncovered some are of single houses, one of adjoining or row houses. It was only when the Assembly or the General Court was in session that the sleepy little village became crowded and the taverns were filled. It was said that the place had many ordinaries charging extraordinary prices. The single houses seem to have resembled the early Williamsburg houses, but with casement windows instead of sashes, some of them covered with tiles instead of shingles.

The hardware of the Jamestown houses differed little from that of Williamsburg. We find the same strap hinges, H-hinges, and butterfly hinges. No doubt they were hammered into shape in the village blacksmith shop. (Fig. 2.)

The tradition that bricks for foundations, walls, and chimneys were brought from England is shaken by the excavating of four kilns. The oldest of these is believed to have been in use as early as 1625. (Fig. 3.)

In the deadly war with the Indians following the massacre of 1622, the English in open battle had the advantage, for the light arrows of the natives made no impression on their coats of plate or mail. In 1625 there were 260 coats of mail with headpieces and 342 sets of armor in the colony. Some of these may have seen service during the Wars of the Roses, for the colonists sent to London for suits of medieval armor from the Tower of London. But the helmet which was dug up about four miles from Jamestown is of the type used during the early years of the seventeenth century; the breastplate, found in a refuse pit, dates from the same period. (Fig. 4.)

Several wells have been found at Jamestown, some cased with brick, some with timbers, some uncased and having a barrel at the bottom. That

FIG. 4. An early helmet found several miles down river from Jamestown, on the north side. Courtesy of the National Park Service.

FIG. 5. A brick lined well. Courtesy of the National Park Service.

FIG. 6. A selection of adzes, axes, and other tools. Courtesy of the National Park Service.

FIG. 7. Fragments of leather shoes. Courtesy of the National Park Service.

the necessity of keeping these wells uncontaminated was understood is shown by the provision in the law that "no man or woman . . . dare to wash any unclean linnen . . . within the pallizadoes . . . nor rench and make clean any kettel, pot or pan . . . within twenty foot of the old well . . . upon pain of whipping." (Fig. 5.)

FIG. 8. A selection of bottles and bottle seals. Courtesy of the National Park Service.

In England the depletion of the forests before the end of the sixteenth century was viewed with alarm, since wood was needed for shipbuilding, for houses, and for potash essential for the making of woolens. In Virginia the forests were an impediment to prosperity, since it was only by arduous labor that openings could be cleared for tobacco fields. So it was to be expected that many felling axes, hewing axes, and wedges would be found. All of the early axes were weighted on the cutting side, for it was only decades later that the colonists discovered that the driving power of the axe would be greatly increased by weighting the blunt side. (Fig. 6.)

Innumerable articles have been unearthed which throw light on the everyday life of the people—scissors, pins, needles, thimbles, candlesticks, snuffers, tongs, andirons, iron pots, trammels, ladles, pans, knives, pot hooks, forks, spoons, earthenware, buckles, buttons, carpenters' tools, blacksmiths' tools, boatbuilding tools, ovens, tools for cultivating tobacco, fishhooks, stirrups, horseshoes, book clasps, etc.

The leather shoe found in an early seventeenth century well was probably imported from England. But it is possible that it was made at Jamestown, for there is evidence of many shoemakers in colonial Virginia. (Fig. 7.)

Probably the first factory in the American colonies was the little glassworks at Jamestown, established in 1608. A small melting pot, part of a large pot, and fragments of green glass have been found on the spot. The glass fragments may have been parts of bottles or of windowpanes. After a second attempt in 1621 had proved a failure, the works seem to have been abandoned. It was easier and cheaper to import glassware from England. (Fig. 8.)

The people of Jamestown, occupying this outpost of civilization, surrounded by the vast forest, threatened by the Spaniards and the Indians, often turned their thoughts to the cities, villages, and green fields of the mother country. So there was joyful excitement when a vessel from England came up the river and tied up at the wharf. The people must have gathered around as bales of goods were unloaded and hogsheads of tobacco taken on. Newcomers were greeted; there were many questions as to what was happening at home; letters were opened eagerly.

Three hundred and fifty years ago the adventurers under Captain Newport were on the broad Atlantic, headed for none know what. Perhaps it was just as well that they did not know, for within a few years most of them were dead, the victims of scurvy, dysentery, malaria, the plague; or of the cruelty of despotic rulers; or of the Indian tomahawk. The advance guard of an army always occupies a position of danger; the advance guard of the millions of Englishmen who went out to the far places of the world paid a terrible price. It is appropriate that in 1957, after the lapse of three and a half centuries, the nation should be doing them honor.

ICELAND AND ITS LITERATURE

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(*Read April 26, 1957*)

LISTENING to Classical and Oriental archaeologists describing their marvelous finds: cities full of art and clay tablet libraries, deserts full of papyri, caves full of Biblical manuscripts, an Icelander is filled with envy. How many manuscripts could Iceland not have if they had not rotted away in a nasty wet climate? There is no hope of finding even small fragments of old vellum, but some of recovering old texts from young paper manuscripts.

Though Iceland may have looked inviting to the Norwegians who settled there in the last quarter of the ninth century, it had neither wood nor stone suitable for building. Hence the isolated farmsteads were built of sod, stones, and rafters, remaining inadequate and perishable up to the present day. Hence, too, archaeology shows that the Icelanders never got so rich as their Scandinavian brethren; there is no parallel to the rich art treasures of the royal Norwegian Osebergship. Also, the number of Icelanders never exceeded eighty thousand and this could be cut in two or more by volcanic eruptions, hard years, and plagues. In fact, the Icelanders might well have gone the way of all flesh like their brothers in Greenland.

One would not guess that this small colony should take the lead among the Scandinavian countries not only in preserving old Germanic-Scandinavian cultural values like the old heathen mythology and wisdom as well as the heroic legends, but also to become entertainers and poets to courts all over Scandinavia and the British Isles up to 1300 and, finally, to write the unique Icelandic sagas. But this is what they did and but for their writings the prehistory of Scandinavia, Greenland, and Vinland would be archaeology only.

Though no Icelandic literature was written down earlier than the twelfth century, it has preserved clear and dependable accounts of the settlement of the country. Iceland was discovered by Norse Vikings sometime near the middle of the ninth century, but the first settler from the fjords in West Norway settled in Reykjarvik in the year 874. Within the next sixty years the country was rather completely settled by Norsemen who came in part directly from Norway and in part after having spent some time in the Norse settlements of the British Isles: Shetland, the Orkneys, the Hebrides, the western shores of England and Scotland, and the East coast of Ireland. We are told that the main reason for the exodus was political. In the last quarter of the ninth century a king had arisen in Norway with the ambition to subjugate the many independent kings, chieftains, and free farmers under his rule, as had already been done by the kings in Sweden, Denmark, and England. This king succeeded in unifying Norway, but many of the chiefs and farmers preferred to leave rather than to submit to his tyranny. Some of them went directly to Iceland, while others went first to the older settlements in the British Isles, settling there as Vikings and venting their wrath for the tyrant by raiding the shores of Norway. The king's answer was an expedition to put down the troublemakers in the West, an event which resulted in a fresh stream of immigrants from these parts to Iceland.

The nature of this emigration had one major effect upon the new population of Iceland: it made for a high proportion of men of stubborn independence and noble birth, though these highborn people also imported slaves from Ireland. There was, indeed, enough intermarriage with the Irish to affect the national stock of Icelanders. Some scholars think that the Irish bards served as useful patterns for the Icelandic skalds or court-poets, who for centuries were to monopolize the Scandinavian courts. But the fact remains that the two types of poetry which the settlers brought to Iceland, the Eddic and the skaldic, both originated in Norway. The Eddic poetry, mythical, gnomic, and heroic, employed the common Germanic alliterative line, familiar from the Old High German *Hildebrandslied* and *Beowulf*. The skaldic verse was also alliterative, but counted syllables, used inrime and assonance and a very artificial diction.

The settlers of Iceland took with them not only a typical new Viking ideology, acquired in the West, but also old-fashioned family solidarity and

ancient and primitive pauper philosophy from Norway. We find the latter in *Hávamál*, the greatest gnomic poem of the *Poetic Edda*. Like Achilles in the *Odyssey* the Norseman did not look forward to death or life after death. It is far better to be maimed than dead, a lame man can ride a horse, a deaf man can fight, alive a man can somehow get a cow. Begging was detested, independence encouraged: having two goats and a brush-covered hut was better than to go begging. In India, Ghandi could survive on one goat; in America the rugged individualists would have approved this way of life; it is wholesome for empty-handed settlers. The first commandment of family solidarity was never to kill a kinsman; save the fighting for outsiders. A man shorn of family ties, loving no one, could feel like an isolated fir tree ready to wither; why should he want to live? Gold and women are the mighty tempters for man to break the family bonds; both will turn him into a traitor; treachery is a terrible offence. These family tragedies happen to the gods themselves in *Völuspá* no less than to the greatest of heroes, like Sigurd the Volsung, and both are subject to inexorable fate. In *Völuspá* the end of the world is foreshadowed by increased incest; conversely the gods returning to the new world rising from the waves make one happy family. This may be the reason why, as Andreas Heusler puts it, the Icelandic literature, rather than that of Christian influence, is the chastest one on earth.

Most settlers coming to Iceland were heathen and remained so for over a century (874–1000). They worshipped several gods and goddesses, among which the fertility god Freyr, the farmers' and seafarers' friend Þórr, and the poets' and warriors' master Óðinn were most important. Doubtless the piety towards these gods was different according to personal temperaments, as illustrated by the two first settlers, Ingólfr and Hjörleifr, the former being pious, the latter not. To the Viking, Óðinn might embody the ideal of enterprising and vigorous youth or the ruthless warlord as compared to the sluggish stay-at-home Þórr. But Þórr was not to be sneered at when it came to defending the world from hostile forces beyond the horizon, like the ever more threatening White-Christ. In general the unrest of the Viking age can hardly have favored conservative piety, and we hear of several men believing in their own "might and main" probably not pious. But one might choose to worship the deity which appealed specially to one's personal fancy, and a free friendship often developed between god and man. This tolerance was extended even to White-Christ, until his intolerant worshippers began to threaten the heathen social order.

One thing is clear: believing in certain gods was not a driving force in the ideology of the Vikings. What was it then? Over and over again we hear that the very un-Christianlike motive for their actions was "to gain fee and fame." On a Viking raid both could be gained, the first by victory, the second by bravery. At home fame and loyalty alike were won by generous gifts to followers. Bravery and generosity were thus cardinal virtues, without which no fame was gained, however great a hoard a man might have amassed. But—and here we come to a cardinal point—a life without fame was not worth living, for it was fame alone in the admiring judgment of the survivors that lasted beyond death.

To win this everlasting fame one must lead a life of honor, in personal freedom, subject to no one, but closely hedged in by conventional morality or public opinion. When Rollo (Göngu-Hrólfr) arrived in France with his Vikings, he was asked: "Who is your king?" His answer was: "We have no king; we are all equals." The heroic code demanded not only generosity toward kinsmen, friends, and guests, but also scrupulous watchfulness against all infringements on one's personal rights or those of one's kinsmen. If such offences were not avenged, the honor was forever tarnished, but death was always preferable to a life of shame.

With such ideals it was natural that the sagas should contain little but feuds and fights between rugged individualists and sticklers for honor. The resulting slaughter could only be deplored by the wiser pagans and considered wicked from a Christian point of view. Conversely the Icelandic admirers of the sagas would have little use for the Gospels: "The Gospels are no fun, there is no fighting in them."

No doubt, the chieftains who sought the security, the pastures, and the game reserves of Iceland had high hopes of being able to lead heroic lives out there, each shipowner at the head of his family, dependents, and slaves. At first he could take extensive lands, settling down as a temple-chief (*goði*) in their midst and distributing the remainder to his followers who owed him personal allegiance for his protection. The chief (*goði*) was normally of noble birth, his power was called chieftaincy (*goðorð*) and his retainers thingmen, because they went to meetings (*þing*) with him.

He often built a temple (*hof*) at his home, his retainers paying a nominal tribute to it (*hoftollr*).

But in Iceland there were no aboriginal natives or peasants to be enslaved or exploited as in England and France, nor could the chiefs expect their fellow settlers to pay the tribute they had left Norway to avoid. The chief thus had to rest content to be *primus inter pares* working and living off his own land like every one else. The ambition of the smaller farmers must have been satisfied, if they were allowed to farm their land in peace and raise a new family where the old family ties had been cut by the emigration. They may have hoped fervently to restore the clannish family solidarity, which speaks so eloquently from Old Icelandic law codes on killing and wergild. But that was apparently out of the question. Instead, they organized themselves into local units (*hreppar*), roughly corresponding to American "townships," of twenty privately owned farmsteads or more, which had fishing rights and pasture lands in common, took care of the poor and infirm, and mutually insured their live stock and the farmhouses against loss or fire—all to a certain degree. It seems that it was more expensive to live on a big estate with many slaves than to turn the slaves into independent cotters.

After sixty years of settlement the chiefs (*goðar*) felt that some organization was needed to equalize their own powers and to curb lawlessness in the land. Thirty-six chieftaincies (*goðorð*) were authorized by law and the incumbents were to meet every summer at Þingvellir to judge cases as they arose and lay down the law of the land. Thus in 930 was founded the unique Icelandic Parliament or Althing (*Alþingi*), which, while the Commonwealth lasted (930–1262), was to act as a supreme lawgiver and court in the country—while each individual, supported by his chief (*goði*) and his friends, had to act as his own executive in carrying out the law. There was only one public official, the Law-speaker (*lögsögumaðr*), whose task it was to recite from memory the whole body of the law in three consecutive summers.

The Althing was formed by the chiefs as a kind of League of Noble Families in Iceland and it is clear that these families were in many ways the cultural torchbearers in Iceland. They cherished genealogy and family traditions both in verse and anecdote. They must have been as attached to the old heroic poetry as the American puritans to their Bible. The temple chiefs must have cultivated mythical poetry for religious purposes. All chiefs were glad to be praised by a skald, though the ambitions of skalds was to praise kings primarily. But no single cause is likely to have contributed more to the turning of the settlers into poets and historians than the uprooting from the old country.

It is hard to overestimate the appetite developing on the isolated farmsteads for news from everywhere. The guests were so welcome, that, as in Ireland, some people built their houses across the road to catch the wayfarer. But nowhere could this hunger for news—from kinsmen in other districts, from relatives abroad, and from the wide world—be as easily satisfied as at the Althing. Here hundreds of the best people from all over the island turned the lava plains and ravines of Þingvellir into a busy and gay capital during two weeks of the brightest and longest summer days, that is, if the gods gave the good weather. Here was ample audience for men who had something to tell, and men with intellectual curiosity had a chance to exchange views and learning. To mention one quoted instance only: here a young Icelandic story-teller learned about the Varangian expeditions of King Haraldr harðráði of Norway from the king's own follower or retainer, Halldór Snorrason, who told the story at the Althing in successive summers.

Most important of all the acts of the Althing was the introduction of Christianity in the year 1000. The two factions had almost clashed in fight, when the moderate leader of the Christians asked the leader of the heathen to decide the issue. After mature consideration he ruled that all the people should be baptized Christians, but one could set out children, sacrifice to heathen gods, and eat horsemeat in secret.

Though, as Toynbee says, this was really the beginning of the end for the Icelandic culture, this compromise Christianity seems to have been beneficial for the rise of the Icelandic literature. In adopting it the old chiefs took care to exercise their old power over the new religion. The missionaries actually asked them to build churches and provide priests for them, which they did, either by having their own sons educated for orders or by "beating" a poor but bright boy "to the book." In the latter case the priest was little more than a servant. This was deplorable from the point of view of the church, but there is little doubt that the Icelanders owe the preservation and composition of their native literature to that very condition. By teaching the chief and their sons to read and write their own mother tongue, as was customary in pre-Conquest England, the

weak church gave them the needed impulse to start writing in the vernacular. Had the church been stronger, as it was elsewhere, it might have produced Latin writers (*cf.* the Danish Saxo Grammaticus) but it would have killed off the native growth. In that case we should have had no Eddas, no skaldic poetry, and no sagas.

Though the clerics learned to write from allegorical homilies and saints' lives, only the missionary kings' sagas were written from a hagiographic point of view. Most of the sagas were actually written from the un-Christian, old heroic point of view of the chieftains, but broadened by the fact that the settlement had acted as a social equalizer; slavery had decreased and there was no shame of manual work any more than in America. From this heroic-social or aristo-democratic point of view the saga writers tried to describe the members of their own society—where everyone knew each other—as realistically as possible in a style as near to every day speech and as far removed from poetry as possible, but dignified, never vulgar, quite free from Christian prejudices and superstitions.

Thus the sagas became a realistic and humanistic island in the ocean of Western Romance and Allegory, quite unknown and little appreciated until the advent of Romanticism and Realism in nineteenth-century Europe. They may come nearer to being a slice of heroic life than many of the European realistic writings, often problem novels and socialist propaganda. The sagas were not propaganda but nostalgic descriptions of bygone or passing life.

The classlessness of the sagas endeared them to the Victorian socialist William Morris. Recently the English poet W. H. Auden has been moved to define the sagas as social realism, not to be confused with the Russian variety. Mr. Auden writes in the *New Statesman and Nation* on November 3, 1956:

> The Icelandic sagas are one of the most extraordinary phenomena in cultural history, not because they are good but because of the kind of goodness which they exhibit. Had the Icelanders of the Middle Ages gone on writing verse epics like the *Battle of Maldon* or turned to ballads, we might admire them as much, but we should not be so astonished. What they actually did, however, was to produce a socially realist literature centuries before any other part of Europe even attempted such a thing and of a quality which, within its limited scale, has never been surpassed.

For further information on the sagas I refer the readers to a forthcoming book of mine, *A History of Icelandic Literature 874–1955*, to be published this summer by the American-Scandinavian Foundation and the Johns Hopkins Press. In the preparation of this book a grant by the American Philosophical Society enabled me to collect material in Iceland.

NESTOR'S PYLOS

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(*Read November 16, 1956*)

After five annual campaigns of excavation the task of uncovering the Mycenaean palace which was first discovered in 1939 at a place called Epáno Englianós in southern Triphylia has been almost completed. This work, carried out under the sponsorship of the University of Cincinnati with funds generously provided by Professor and Mrs. W. T. Semple, forms the American part of a joint Helleno-American project for the archaeological exploration of southwestern Messenia in which I have had as a colleague Professor Sp. Marinatos of the University of Athens. A report on the results of the first and second seasons was read before the American Philosophical Society on November 15, 1953. It is a great privilege to have the opportunity to present a brief survey of what has since been accomplished (fig. 1).

The progress of the excavation has made it clear that the palace consists of two distinct units or insulae, separated one from the other by an open court, and almost surely representing two periods of construction.

What we take to be the older of the two units occupies the southwestern end of the hilltop (fig. 2). The greater part of what has survived was exposed under the supervision of Mrs. Blegen in 1955 and 1956. The floors have suffered much from erosion, and in many places only the stumps of the walls remain, or merely the deepest foundations. Nevertheless, the ground plan is in its

Fig. 2. General view of southwestern section of palace, from east. (Photograph by Alison Frantz.)

Fig. 1. General view across palace from north: in foreground northeastern wing, in middle-ground megaron complex, in background older wing; in distance Bay of Navarino and Island of Sphacteria. (Photograph by Alison Frantz.)

Fig. 3. Older wing of palace, from north, looking across court with apartments of state in center background, domestic quarters at right. (Photograph by Alison Frantz.)

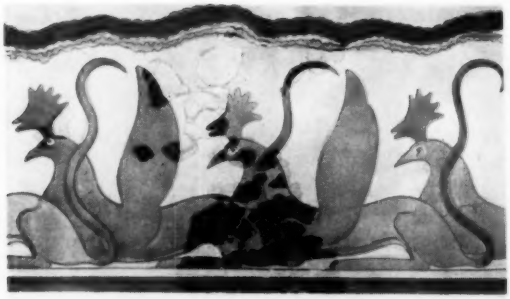

FIG. 4. Fresco of winged griffins from hall in older wing of palace. (Water color by Piet de Jong.)

main lines recognizable with all its principal divisions (fig. 3). The extreme western angle of the building has not yet been fully excavated, but the southwestern unit as a whole measures approximately 32.50 m. long by 25 m. wide. It comprised at least eighteen halls, rooms, corridors, courts, and a stairway. Since most of these divisions were evidently duplicated in an upper story, the earlier palace was certainly of respectable size and capacity.

The apartments of state may be recognized in two relatively spacious halls. One, with a two-columned façade opening southeastward on a large court, had a single, axially placed interior column of wood to help support ceiling and roof. The walls were plastered and elaborately decorated with frescoes, one composition showing a row of winged griffins (fig. 4). From this room, passing through a doorway flanked by a sentry stand, one could enter a lateral apartment of about the same size (some 7 m. by 10 m.). Here there seem to have been four fluted wooden columns, possibly enclosing a central hearth, and this was in all likelihood the ceremonial throne room. The plan differs notably from that of the orthodox Mycenaean megaron as exemplified in the later, northeastern insula.

FIG. 6. General view of megaron complex looking northeastward across vestibule to stairway. Mount Aigalion in distance. (Photograph by Alison Frantz.)

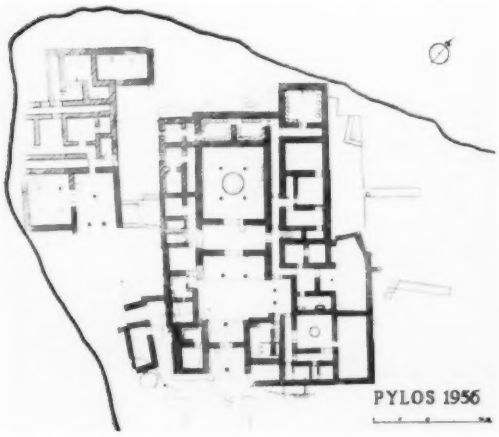

FIG. 5. Plan of palace at end of fifth campaign. (Measured and drawn by Demetrios Theocharis with additions by Piet de Jong.)

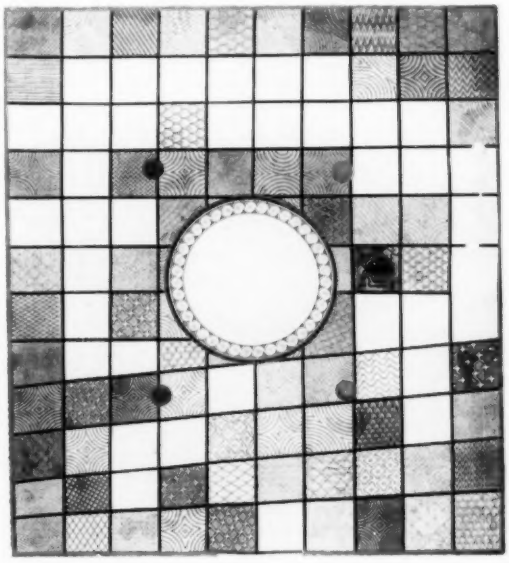

FIG. 7. Painted stucco pavement and central ceremonial hearth of Throne Room. (Water color by Piet de Jong.)

FIG. 8. Fresco behind throne, a reconstruction as envisioned by Piet de Jong.

The latter was much larger than the wing we have just considered. Here we have a striking regularity of plan (fig. 5), obviously the creation of a skilled professional architect who drew up his design in the Mycenaean manner distinctive of the Greek mainland (fig. 6): from a broad exterior court a formal Propylon, with a single column between antae in each façade, leads to a smaller inner court; thence one passes straight ahead through a distyle Portico and a shallow Vestibule into a stately Throne Room with decorated floor (fig. 7), painted central hearth and brightly frescoed walls (fig. 8). Four nearly symmetrically spaced wooden columns standing on stone bases probably supported a balcony as well as ceiling and roof (fig. 9).

Surrounding this axial core of the palace on the

FIG. 10. Stone stairway to upper floor in northeastern part of palace. From southwest.

southwest, northwest, and northeast are archives rooms, waiting rooms, pantries, magazines, many storerooms, a bathroom, and in the eastern angle apartments for the queen and the ladies of the establishment. Remains of three stone stairways have been found, one on the northeast (fig. 10), one on the southwest, and one on the southeast, ascending to an upper story in which the plan of the ground floor was doubtless in the main repeated. The northeastern wing would then have contained nearly 100 rooms of one kind and another.

The eastern quarter of this wing (fig. 11) was

FIG. 9. General view across palace from northwest: in foreground, magazines; beyond, Throne Room with hearth; at left, northeastern section with storerooms; at right, pantries with court beyond. (Photograph by Alison Frantz.)

FIG. 11. General view looking across Propylon to eastern quarter of palace. From southwest. (Photograph by Alison Frantz.)

FIG. 12. View across bathroom and porch. From northeast. (Photograph by Alison Frantz.)

exposed to view during the seasons of 1955 and 1956 under the supervision of Miss Marion Rawson. A northeast portico came to light facing the court in front of the Throne Room. The two wooden columns set on stone bases had been consumed by the fire that destroyed the palace, but impressions in the base-ring and floor of stucco indicate that each shaft was delicately fluted, with 60 narrow vertical channels.

FIG. 13. Terracotta bath set in stuccoed supporting tub of clay. (Photograph by Alison Frantz.)

Behind the porch, the upper floor of which offered a fine point of vantage for viewing ceremonies in the court to the southwest, a corridor led to a lobby whence access could be gained to a bathroom (fig. 12), where a built-in terracotta bathtub was found still in place, set along one wall, fitted into a sheath-like container of clay or crude brick (fig. 13). The terracotta tub bears painted patterns both inside and outside, and was thus evidently not originally meant to have its decoration covered over with clay. In the corner of the room to the southwest two large storage jars—presumably for water, possibly for wine—had been set into a high stucco-coated stand made of clay. A plain stemmed drinking cup lay on the bottom of the bathtub; seven intact kylixes of the same kind were found in one of the jars, nine broken examples in the other. Was it perhaps the custom of the occupants of the palace to fortify themselves with a drink of wine while they were being bathed? That is of course a possible explanation, but the cups may likewise or alternatively have been used as containers for oil or perfume.

FIG. 14. Queen's apartments from southeast. In foreground, foundations of southeast wall; beyond, from left to right, long corridor, washroom, narrow corridor, boudoir. In background, hall with hearth. (Photograph by Alison Frantz.)

However that may be, it was no doubt in a bathroom such as this one that Odysseus' son, the young Telemachus, was bathed as told in the *Odyssey*. As a special honor, King Nestor ordered his youngest daughter, Polykaste, to give the young man his bath, and when she had rubbed him down, anointed him with oil, and covered him with a handsome cloak, he came out into the court looking like one of the immortal gods.

Beyond a thick wall to the southeast of the bathroom, and accessible through a corridor opening from the northeast portico, Miss Rawson discovered what we may unhesitatingly recognize as the special apartments of the queen (fig. 14). The principal room is a fairly large one, measuring more than 6 m. on a side, entered through a broad doorway from the corridor. In the center of the room is a raised circular hearth coated with stucco, like the one in the Throne Room, but much smaller. Four successive coats of stucco can be distinguished, each bearing painted decoration: a flame pattern, a saw-tooth motive, and a broad band of running spirals bordering the top of the hearth. The floor of the room was probably once decorated, but the intensity of the fire that wrecked the building has turned the surface of the stucco

FIG. 15. Queen's apartments: Floor decoration in boudoir. (Water color by Piet de Jong.)

pavement into powdery lime. Innumerable fragments of frescoes fallen from the walls were collected, and one piece of plaster, retaining remnants of painted decoration, was found still adhering to the southwestern wall of the room. Further cleaning and study of the material will be required before we can determine the subject of the composition.

On and about the hearth were recovered fragments of two large pipes of terracotta, about 0.55 m. in diameter. They obviously belong to a chimney, meant to carry off the smoke from the hearth, a device like that we already know from the Throne Room.

From the queen's reception hall one doorway leads northeastward to a small walled, but unroofed, court; another opens southeastward into a corridor from which, by a narrow branching passage, one reaches a diminutive boudoir. The floor, both in the passage and in the boudoir, was marked out by incised lines into squares bearing painted decoration: abstract linear patterns in an outer border, octopuses and dolphins, or other fish, in the squares inside the frame (fig. 15). The walls had been decorated with paintings, but only fallen fragments have survived.

Southwestward beyond the boudoir and the passage, but accessible only through the main corridor on the southwest, is yet another small room belonging to the queen's apartments. It has suffered irreparable damage from the fire so often mentioned, but it was once adorned with wall paintings and probably had a decorated floor. A pierced stone slab set in the pavement served to carry off water into an underground drain, and this simple plumbing probably marks the little chamber as a washroom. More than a dozen fairly large stirrup-vases, all badly shattered, were found on the floor of the room. They had no doubt served some purpose connected with ablutions, perhaps for pouring water over the hands in a slow trickle in the fashion of the Near and Middle East.

Exactly how long a time elapsed between the building of the earlier wing and the later wing of the palace has not yet been surely determined, though we believe the interval was relatively short. In any event, once the whole structure was completed, both wings were occupied until the entire palace perished in a tremendous conflagration which left in its wake a heap of charred and calcined rubbish that was never again reoccupied by human habitations.

Precise dating for this early period is still somewhat hazardous. But in comparative terms we may safely say that the destruction took place at a time when pottery in the style of Mycenaean III B was giving way before that of Mycenaean III C. Vast numbers of pots have been recovered from ten overflowing pantries and from the floors of many other rooms in the palace. Much of this material, representing vessels that were in use down to the very day the building was burned, must in our judgment be attributed to the category designated as Mycenaean III B (fig. 16). But there are also a good many pieces assignable stylistically to Mycenaean III C, and a few that belong unmistakably to the so-called Granary Class (fig. 17). An exact chronology in absolute terms for these styles of pottery has yet to be established—the evidence is still inadequate. Meanwhile we have suggested—in round numbers—

FIG. 16. Painted pots in style of Mycenaean III B. (Water colors by Piet de Jong.)

that the burning of the palace took place about 1200 B.C.

Whatever the absolute dates may ultimately turn out to be, the end of the palace may with some confidence be chronologically fixed in relative terms as compared with the settlements at Troy. Troy VIIa which can now be shown to have been destroyed by fire accompanied by violence, and which we believe to be the Troy of Priam and of Homer, has yielded not a little imported Mycenaean pottery—some in the style of Mycenaean III A, much more that must be classed as Mycenaean III B; but so far as we could determine, there is not a single fragment definitely assignable to Mycenaean III C. It thus appears that the end of the Trojan War preceded by some time the destruction of the Mycenaean palace in Messenia. The war of the confederated Achaeans against Troy must in any case go back to a stage when the Mycenaean III B style prevailed and all the Mycenaean settlements throughout the Greek mainland seem to have flourished most prosperously.

This chain of events fits well with the sequence as we find it recorded in Greek tradition. When the Trojan War came to its end King Nestor with his contingent of ships returned to his domain in Messenia, where he continued to rule for some time in his old age. He was succeeded on the throne in turn by a son and a grandson before the palace was captured and burned by the invading Dorians. The occupants fled before the irresistible onslaught, some seeking refuge in Athens, others continuing on across the Aegean to Ionia. The palace, sacked and burned to the ground, was left a ruin, and the hill was henceforth abandoned. Gradually the debris became covered with earth and vegetation and eventually perhaps an olive grove took possession, effectively concealing the underlying remains from view. In classical times the location of the site had been altogether forgotten, and no one knew exactly where the palace had stood. Thucydides seems to have had no doubts that it was somewhere in Western Messenia, but he makes no attempt to identify the spot more specifically.

The problem later became a much debated academic question, and in the first century B.C., as Strabo reports, three different theories concerning the original site of the palace had been evolved. These theories were based not on archaeological evidence but on interpretations of passages in Homer and other early poets. One view insisted that the ancient town had stood close beside the sea, presumably on the height that in the fifth century was called Pylos by the Messenians, but was known to the Spartans as Koryphasion. A second

FIG. 17. Painted pots in style of Mycenaean III C. (Water colors by Piet de Jong.)

conjecture placed the site farther inland, somewhere among the foothills of Mount Aigalion. The third theory, propounded by the "Homerikoteroi," the Homeric scholars (with whom Strabo himself agreed), asserted that the long lost capital of Nestor must be sought far to the north near the Alpheios River in ancient Triphylia.

Through the discovery and uncovering of the palace at Epáno Englianós this long disputed academic problem in topography has at last found a convincing solution, favoring the second of the three views mentioned.

As was apparent almost from the outset of the excavation, the dominating position and the size of the building, its architectural distinction in general and in detail, the spaciousness and quality of its Throne Room, the character of its floor decoration and its wall paintings, the evidence of economic wealth and political power—all these factors place the new palace as an equal alongside its compeers at Mycenae and Tiryns. In their chronological setting and in all other essential respects the three stand together. It is true that the surrounding fortification walls, so striking a feature at Tiryns and Mycenae, are apparently lacking in Messenia. Their absence might suggest that a greater confidence in external and internal security prevailed in Messenia than in Argolis.

The hope that, following the brilliant discovery by Michael Ventris of the key to the Linear B syllabary, the reading of the Pylos tablets might reveal the name of the king who sat on the throne in our palace, has not yet been realized. On the tablets the monarch is regularly called merely "the king," and in only a single instance is there a doubtful reference to an actual name (Echelaos, otherwise unknown). This name, if correctly read, gives little help toward identifying the dynastic family. In any event, since the tablets deal with bookkeeping accounts presumably of the current year in which the building was destroyed, the name of the ruler can obviously not be that of the king who built the palace, but must belong to a successor two or three generations later.

Despite some minor points that still remain uncertain in connection with the problem of identification, it seems to me that the general evidence is simple, clear, and decisive for the settlement of the question. Our palace is obviously a capitol worthy to stand alongside the palaces at Mycenae and Tiryns. In the Greek epic, in tradition, and history the only royal family in Messenia that had the wealth and the political power to hold its place beside the Atreids and other leading Achaean chieftains was that of the Neleids and its most illustrious scion Nestor.

THE NATION'S RESEARCH AND DEVELOPMENT—THEIR DEFICIENCIES AND MEANS FOR CORRECTION

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President, Bell Telephone Laboratories, Inc.

(*Read April 26, 1957*)

THE expenditures of the nation in 1956 for research and development were some $7.0 billion—less than two per cent of the Gross National Product. Approximately 325,000 scientists and engineers were in the program. The growth since 1940 has been unprecedented, some ninefold. At that time the nation's level of expenditures was only some $800 million in dollars of today.

That research and development is the life blood of an industrial society and its military organization is a generally accepted fact. It has converted our nation from an agricultural to an industrial society in the short period of fifty years. This conversion has given our young nation an economic strength superior to all others. In like manner research and development has provided us with the most powerful military system of all time. It has industrialized warfare. War is no longer a contest of human strength, but of industrial might. A nation cannot now be superior in war unless it is superior in industrial strength.

Lest I appear to place too much emphasis on power, may I remind you that increasing economic strength provides expanding opportunities for cultural development. More abundant material comforts and increased time for leisure are among the rewards of a strong economy that is protected by adequate military power.

With the unprecedented contributions of research and development to economic and military strength in the post-war years, one could well question the need for stressing the deficiencies in research and development. The deficiencies are indeed large and the values at stake in their correction are vital to the nation and to all free peoples. I believe that this is one of the most important problems—perhaps the most important problem in a long range sense—that faces the nation. It is for this reason that those with professional knowledge of research and development have a solemn obligation to bring these deficiencies to general attention and to suggest avenues for their correction.

The dramatic economic and military progress that Communist Russia is making threatens, in the long range, our number one position in world strength. This, coupled with their invariant determination to dominate the world, with all that would follow should they succeed, affords adequate basis for our nation's exerting itself to the utmost to maintain its superior position; for it is only in this position that we may be able to preserve, without war, the freedoms of Western man. Strength in research and development is of vital importance to the maintenance of our number one position of power.

The major deficiencies in the nation's research and development can be divided into three general classes.

Our educational system: It is not providing adequate training for careers in research and development and is not attracting to such careers a large enough fraction of those with latent talent.

An imbalance among the components of research and development: Too small a fraction of the nation's research and development effort is in basic or pure research.

Inefficient use of manpower in the program: Research strength is dissipated and excessive manpower is consumed.

There is large popular emphasis on the shortage of scientists and engineers. To be sure, there is a shortage but it will have less significance as the effectiveness of our effort increases. The inadequacy of the financial support of our educational system is also stressed. The inadequacy is indeed large, however, and, unless we eliminate or materially decrease the deficiencies in our educational system, added money will not provide a solution.

THE EDUCATIONAL SYSTEM

At the secondary and high school levels of our educational system, the science training is inadequate in quantity and quality to attract a sufficient number of our talented youth into careers. A relatively small fraction of our young people receive training in the specific disciplines of

physics, chemistry, and mathematics. So-called courses in general science have been substituted in many schools. Even where training is provided in the specific disciplines, the quality of the instruction is too often low.

If the nation is to develop a strength in research and development that is limited only by the latent capacities of its people, our pre-college training must be such that a much larger per cent of our young people of superior talent for science are attracted to it as a profession, and obtain a level of training limited only by their capacity to absorb. We are now falling far short of both of these goals.

I recently joined with a group of academic and industrial physicists in a review of a few of the popular high school texts in physics. We were shocked at our findings. The texts are dominated by stories of industrial and military applications. The principles of physics are so dwarfed by a mass of qualitative applications information that basic subject matter is all but lost. Many of the teachers are so poorly trained in science that, even were the texts and laboratory facilities adequate, the instruction would not be inspiring or effective.

The nation's schools of education, I believe, are primarily responsible for this situation. They have insulated themselves from the ever expanding forefront of knowledge in science and directed too much of their attention to the methodology of teaching and too little to the substance of the material to be taught.

One must conclude, from the educational pattern they have developed, that their chief concern has been the adjustment of our children to life in our society, to the neglect of the identification and development of the talented. The preservation of our society and the continuing enrichment of its culture in the next generation depend on its talented children of today. Alas, we are now neglecting their identification and development.

Those of us in the sciences with professional knowledge of its needs must also share the responsibility. We have, largely, stood idly by and let the schools of education determine the philosophies and practices in the science education of our children.

Our society generally must also share the responsibility for this situation. It has provided neither an adequate level of financial support nor the recognition that science's value warrants. It has placed so low an evaluation on the worth of pre-college teaching that the level of compensation is causing a deterioration in the quality of the teaching staffs. Unless the compensation for science teaching in our pre-college tax-supported schools is materially increased, a competent corps of teachers cannot be maintained. For some time, school construction has been inadequate. There is now overcrowding in many of our schools and new construction for the rapidly increasing school population is lagging.

We shall now turn to the professional and graduate school level of training. At least two-thirds of the professional participants in research and development are engineers. It is only in the post-war years that engineers have participated in research and development in large numbers. Previously most were employed in design, construction, manufacture, and in the service industries. Curricula were largely patterned for these careers, with emphasis on current engineering practice and limited attention to mathematics and science and their application in creating new facilities and practices.

Four years of training for engineers is the traditional national pattern. Only recently have there been a significant number with a longer period of training, and only a few of them have continued to the doctorate level. Even with the largest possible emphasis on science and mathematics, with the increasing depth of science and its requirement of mathematics, adequate training in these subjects cannot be provided in four years. Additional training time is essential.

Russia has established a pattern of engineering training that provides the depth in science and mathematics that is so essential. A yet unpublished report of a study made in Russia last summer by a competent British team, gives detailed information and evaluation. The Russian program is of highest quality. Five and one-half years, in contrast with our four years, is the normal training period. The maturity in science and mathematics of those entering the program far exceeds our norm. The school year is of ten or more months; ours is of nine months. The school week is of six days, in contrast with the five-day week here, and there are more scheduled hours in each day than is our practice. In all the engineering disciplines the depth of training in mathematics and science far exceeds our norm. The narrowness and specialization of the training is the chief criticism of the British team.

The faculties are deeply trained, most competent and enthusiastic. Far too large a number of our engineering faculties have only four or five

years of training. The faculty pay levels exceed those of corresponding technical men in industry. Both are near the top of a national pay range much wider than ours. Faculty compensation here is well below that of corresponding men in industry. In Russia financial rewards or bonuses are given for superior performance in teaching and research. There is largely an absence of special rewards here.

Since the nation's engineering schools must continue to provide the training for more than half of those in its research and development, it is imperative that the training for those choosing careers in creative technology be drastically revised. The minimum period of training should be five years, with a significant number continuing to the doctorate level. Mathematics and science must receive greatly increased emphasis. Adequate preparation for operational engineering, in contrast with creative engineering, can well be obtained in the four-year period. A separation in curricula for the two areas is indicated.

Industry, where about 70 per cent of the nation's research and development is done, can make large contributions to eliminating this deficiency in engineering education. It can establish higher pay levels for the more deeply trained to encourage more students to continue their training. As soon as manpower shortages are eased, industry should discontinue employing, for research and development, men with only four years of training.

Industry can also encourage graduate training on the job. Much of this is now in progress. Bell Telephone Laboratories, for example, instituted an in-hours training program some ten years ago. All of its four-year engineering graduates devote almost half of their first three years to graduate courses in science and mathematics. Their compensation progress is the same as it would be if their full time was spent in the work of the Laboratories. The program has been in progress long enough for an evaluation of worth to be made. Including the half time off the job, but paid for, the cost of the training program is approximately $15,000 per man. We consider it a good investment. The effectiveness of the young men is greatly increased. Their deeper training gives protection against their obsolescence as science advances.

Approximately one-third of the participants in the nation's research and development are trained in science. The nation's training in science to the doctorate level is closely in accord with the needs of a career in research. A large fraction of science students preparing for research careers continue to the doctorate level. There are perhaps fifty universities with graduate programs in science to that level. These programs, while there is a considerable spread in their quality, are in general good. Their median compares favorably with graduate training anywhere in the world. Our ten to fifteen leading graduate schools of science are probably among the best twenty of the world. There is, however, one area of national weakness in the training of our scientists. That is in applied mathematics, in contrast with theoretical physics, which is excellent. The correction of this deficiency is of major importance.

There has been a growing and much needed trend to post-doctoral work in science. At least 20 per cent of those receiving doctorates will benefit from post-doctoral work, as will the nation's strength in science. Some of our foundations, recognizing this need, are providing post-doctoral fellowships adequate for full financial support during the period of study and research. The National Science Foundation has also recently initiated a post-doctoral fellowship program. There is need for a much larger number of these fellowships.

Our engineering and scientific schools, like our secondary schools, are handicapped by inadequate financial support. Faculty compensation is too low. Buildings are now overcrowded and increasing enrollment will bring added demands for housing. The acute shortage of scientists and engineers of the past few years has brought about rapidly increasing pay levels in industry, widening still further the spread between academic and industrial pay levels. This is causing many faculty members to accept positions in industry. Perhaps of even greater long range consequence is the inability of the academic community to obtain its share of the unusually talented young men completing their academic training. Increased financial support at the university and graduate school levels is imperative.

TOO LITTLE BASIC RESEARCH

The volume of basic research is too low and its fraction of the nation's research and development is too small. Not more than 5 per cent of the $7 billion expended in 1956 was for basic research. Professor Glenn Seaborg recently stated that at least twice that percentage could well be spent. With this I am in complete agreement. Basic research provides the knowledge that paces progress in providing new facilities and services

for our society and in increasing the nation's strength. Money spent for basic research is one of the nation's best investments; too little and too late could be of tragic consequence.

Most of the basic research is done in our academic institutions and private foundations. The university is traditionally the home of basic research. It provides the environment and freedoms essential for research to thrive. It must continue as the primary source of new scientific knowledge. Industry, the principal converter of new scientific knowledge into facilities and services for our society, in its own interest as well as that of the nation, must make much larger contributions to basic research.

In the post-war years, basic research in the universities has expanded greatly. Its rate of growth should be increased. Further expansion, however, is limited by lack of funds. These funds must be sharply increased. If the recent rate of growth is to be maintained, faculties must be enlarged, new space provided and operating funds increased.

A typical example of inadequacy of funds, and in a most important area, is that of atomic nuclear research. In the years since 1940, our nation has made the largest contribution to new knowledge of the nucleus; but we yet have a long way to go. Continuing progress in nuclear structure research demands particle accelerators more powerful than any we now have available. A recent study for the National Science Foundation by a committee of distinguished physicists stresses the limitations of present particle accelerators and recommends the prompt construction of several accelerators of higher energy. The cost of construction and operation of these accelerators is very large. The nation is now expending some $40 million annually on research employing the high energy accelerators. The committee recommends that this be increased to $60 to $90 million annually in the next five years.

The high energy physics that these accelerators implement is at this time the most challenging activity in science, both with respect to its technology and its central importance to basic science. The number of high energy accelerator installations in Soviet Russia has increased significantly in recent years. Although our nation is now leading in the number and diversity of its accelerators and in the productivity of its research programs, the rate of Soviet progress has been such that this situation may well not persist through the next decade. The apparently unlimited government support by the Soviet, and the high rate of production of well-qualified scientists and engineers present a challenging situation. Unless our scientists are promptly provided with the facilities and funds for their operation, the loss of our number one position in high energy physics seems inevitable. I have presented this situation as it is typical of many others that are developing. Russia appears determined to surpass us in science and technology and is committing large resources to that end, well in excess of our commitments, and has coupled this with an educational development program for scientists and engineers larger and more effective than ours.

Less than 20 per cent of the nation's basic research is done in industry's laboratories, and this is highly concentrated in a few of them. Seventy per cent of the nation's research and development is done by industry, but less than 2 per cent of this effort is in basic research. This statement of industry's small basic research contribution may be questioned. It is supported, however, by the relative volume of basic research reports by industry at meetings of our scientific societies. Of the 450 papers presented at a recent meeting of the Physical Society, only 46 came from industry and more than half of these were from five laboratories. The data of this meeting are typical.

Industry is the potential source of basic research contributions many-fold greater than its present volume. A few of industry's laboratories of greatest maturity have amply demonstrated that industry can maintain the environment and freedoms essential for productivity in basic research by its scientists. As the director of one of these laboratories, I know that basic research is of great value to the company and to the nation. For the company, it makes possible early invention in new areas of science. It can add great strength to the applied research and development activities of the company. In fact, those large laboratories that do not conduct basic research in an amount in balance with their applied research and development, will not attain the highest possible level of productivity for their applied activities. For the nation, it adds to the total reservoir of its new scientific knowledge and provides more effective laboratories for the development of facilities for the military.

Basic research is a long range undertaking; through it a company is investing in its future. At least five and often ten years elapse before the new knowledge from basic research is translated into industrial products. However, after the ini-

tial period, a new basic research organization of adequate size will provide a continuous flow of new knowledge of great value to the company.

Large resources, patience, and a management with professional understanding are absolutely necessary for success. At least one hundred of our industrial companies have adequate resources and broad enough technologic interests to support significant programs in basic research; yet not more than twenty companies are now doing so. Why is the number so small? There is a complex of reasons. The more important ones are the immaturity of industrial research laboratories and the nation's lack of basic scientists with management experience in industry.

Industrial research is scarcely fifty years old in our country and most of industry's laboratories are not yet twenty years of age. We are, therefore, suffering from immaturity. The scientific and technologic activity, from basic research to design of products, is one of the most complex of the professional activities of our society. It is my judgment that only one who is deeply trained in science and has basic research experience can have the perspective to organize and develop programs extending from basic research through the succeeding technologic steps for an industry. Too many of industry's technologic leaders, while most able men, are engineers and not deeply trained in science nor with basic research experience. If one examines the technical leadership of the nation's successful large industrial companies with laboratories of forty to fifty years of age, he will find a continuity of leaders who are scientists possessing basic research experience.

For building basic research into industry and improving the effectiveness of manpower usage, our greatest need is for professional leadership adequate to encompass the entire scientific and technologic range of research and development. Such leadership can obtain management's support of basic research and can lead in the building of staff, patterns of organization, and environment so essential to balanced success in all sectors of research and development—basic research, applied research, development and design of products for manufacture and use.

INEFFICIENT USE OF MANPOWER IN THE PROGRAM

This discussion will be limited to industry and to the defense activities of the government. More than 80 per cent of the nation's research and development is in these two areas.

As a whole, the nation's industry makes inefficient use of its scientific and technologic manpower. This is principally due to inadequate organization of the effort and to the lack of training in science and mathematics of the engineers who occupy some two-thirds of the professional positions in industry's research and development. Through the inadequacies in organization, many of the scientists in industry lose or greatly reduce their capacity as scientists within a decade after their entrance into industry. An important national asset is lost.

The pattern of programmatic organization of research and development is of primary importance in the effective use of the professional manpower. Basic research in industry can only thrive and be productive in an environment of scientific freedom as closely akin to that of the academic world as industry can provide.

This can best be realized through segregation of basic research—in organization and space—and diluting it with only a limited admixture of applied research. The work should be free of time schedules, with work directed only at acquisition of new knowledge and disassociated from specific development objectives. The research staff can be available for consultation and education of the development staff, to the material profit of both. Such an environment preserves and enhances the scientific and basic research competence of the staff. Where research is intermingled with development, as is frequently done, scientific and basic research competence usually wanes. In not too many years, the scientist becomes a development engineer.

Basic research in industry, when so segregated and staffed with highly competent scientists, can in some ways and in many areas of science be even more productive than basic research in the academic world.

The development and design operations of research and development are most efficiently performed when the development objectives can be realized within existing basic knowledge and art. When research, which cannot be time scheduled, is required in meeting the development objectives, the over-all use of manpower in the development project is inefficient and much more time is required.

It is for this reason that Bell Telephone Laboratories has evolved a most valuable coordinate step between research, basic and applied, and the development and design of the new facilities. This we have called development planning.

Here, a competent team of scientists and development and operating engineers prepare new development plans where objectives that are of value to the Bell Companies or the military can be realized within the framework of existing knowledge and art. If there are gaps in the knowledge or art for a worth-while development, its initiation is delayed until applied research provides the required knowledge or art.

The three coordinate functions—research, basic and applied, development planning, development and design—when translated into an organizational structure suited to the size of the laboratory, provide a framework for the most effective use of manpower and, importantly, for the preservation and enhancement of the professional competence of the variety of specialists involved. I am confident that when a major portion of the nation's industrial research and development is performed in this ordered pattern and when our engineers are more deeply trained in science and mathematics, large manpower economies will result, the end product will improve, professional competence will be preserved, and we shall hear less of manpower shortage.

Some $2 billion is expended in research and development for national defense, 30 per cent of the nation's total. Industry, under contract with the government, carries out more than 60 per cent of the program. The deficiencies of industry's research and development, therefore, apply with the same significance to the military programs as they do to those of the civilian economy. Beyond this, however, are the deficiencies in the organization and operation of the military in their programming and directing the course of these developments. Time does not permit a discussion of them. However, my views are presented in the Report of the Task Force of the Second Hoover Commission, which was concerned with our defense research and development.

Because of time limitation, the discussion of the deficiencies of our educational system has been restricted to their impact on the professional aspects of the nation's research and development. I am well aware of the broader aspects of these deficiencies. Those of our secondary schools, for example, have an impact upon the whole social and economic life of our society.

I have also stressed the limited training in science and mathematics of our engineers entering research and development and ignored their lack of training in the cultural subjects, in the so-called humanities. This lack, coupled with that in science and mathematics, adds emphasis to the need for a longer period of academic training.

I have presented this frank and critical analysis of our nation's research and development because of my deep conviction of its dominating importance in the survival of Western man's freedoms.

Only through large additional expenditures accompanied by major correction in the deficiencies I have discussed can our nation maintain its present preeminent position essential to our preservation without war. To do this in time, there must be general knowledge of the situation accompanied by a change in our society's sense of values. At present, it places far too low a value on education at all levels and on research in the academic world. Without this change in evaluation, all the other necessary actions cannot be effectively taken.

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